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PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED
WITH THE COÖPERATION OF THE

AMERICAN PHYSICAL SOCIETY

BY

EDWARD L. NICHOLS
ERNEST MERRITT, and FREDERICK BEDELL

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JULY, 1910

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THE PHYSICAL REVIEW

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THE

PHYSICAL REVIEW.

A METHOD FOR MEASURING ELLIPTICITY AND THE DETERMINATION OF OPTICAL CONSTANTS OF METALS.

By A. Q. Tool.

THE object of this investigation was twofold, namely: to develop an accurate and convenient half-shade system for measuring the constants of elliptically polarized light and to apply it in determining the optical constants of metals by reflection. Since in every half-shade compensator method it is necessary to determine the orders of the analyzing plates of the system, and also since the orders of these plates vary greatly with the method used, any instrument designed for determining them must have a wide range of applicability. Such an instrument is also valuable since it can be used for measuring ellipticities in general. The instrument to be described was devised with this purpose in view.

I. Description and Use of Instrument.

The various methods employed for obtaining the constants of elliptically polarized light have differed greatly according to the conditions to be met. Babinet's compensator has been used chiefly for large ellipticities, while for small the Bravais bi-plate method with many modifications, earlier, and the Brace elliptic half-shade, more recently, have been employed. A number of years ago Stokes[1] devised a method by which a compensator of unknown order (approximately a quarter-wave plate) is used to restore the light to plane polarization. This possesses the advantage of allowing a

[1] Stokes, G. G., Phil. Mag. (4), Vol. 2, p. 420, 1851.

compensator with considerable differential dispersion to be used throughout the spectrum and at the same time permits a wide range in the magnitude of the ellipticity to be measured. The chief difficulty with this instrument, however, is in setting the compensator and analyzing nicol with sufficient accuracy. The system used in the present investigation was devised to increase the sensibility of Stokes's method. In principle it consists in compensating the light down to a definite small ellipticity as tested by the Brace half-shade, instead of to plane polarized light.

The apparatus was designed after that of Stokes, the compensator being mounted on a circle movable with respect to a fixed vernier, and the analyzing nicol on a second vernier movable with respect to the compensator circle. (The circle and verniers were graduated to read to .01°.) The following modifications, however, were introduced. The analyzer consisted of a Jellett split nicol combined with an elliptic half-shade fixed relatively to the nicol and just before it (*i. e.*, toward the light source). This half-shade is best chosen with its dividing line making an angle of approximately 45° with its principal axis, so that when mounted with its dividing line perpendicular to that of the nicol, its optic axis will be at 45° to the axis of the nicol. A simultaneous "match" of the four parts of the field thus obtained constitutes a "setting". The ocular should therefore focus on both dividing lines at the same time.

The compensator was in most cases a plate of mica of such thickness that its order was approximately 90° for 560 $\mu\mu$. The average order of the two parts of the elliptic half-shade (also of mica) was varied over a wide range (from 0.5° to 30°) but the difference in the order of the two parts was never greater than 6°, nor less than 1°. This difference in order and also the angle between the principal planes of the split nicol (2° in the greater part of this work) were varied according to the intensity of the light tested.

This analyzing system was mounted on one arm of a spectrometer and just before the telescope, which was interchangeable with a short focus ocular—the telescope serving for all spectrometer adjustments and the ocular for matching the analyzing system. The polarizing nicol was mounted immediately after the collimator lens. Monochromatic light (as obtained from a spectral instrument) was

focused on the collimator slit of the spectrometer, the range being within 4 $\mu\mu$ throughout the spectrum.

With monochromatic light and uniform illumination a simultaneous match of three parts of the field in this apparatus gives theoretically a match of all four. With thin half-shades and nearly monochromatic light this condition is realized experimentally, but under working conditions it is sometimes necessary to be satisfied with a match of three fields, which is sufficient provided the same three are always used.

The procedure for obtaining a match is to adjust first the compensator and combined half-shade system to approximately minimum intensity of field, then rotate them together for a match of the elliptic half-shade. With compensator fixed, the half-shade system is then rotated to a match of the nicol, which destroys to a slight extent that of the elliptic half-shade. A simultaneous match can then be obtained by repeating the last two adjustments. Having obtained a match, the positions of the compensator and nicol (R and r respectively) are observed and likewise for the complementary match (R' and r').[1] As in Stokes's method the direction of the major axis of the ellipse is given by $[(R + R')/2 - \pi/4]$. Tuckerman[2] has given the theory of this instrument in his paper on Elliptic Analyzing Systems, in which he gives two methods for calibrating the half-shade and compensator. First, when *the initial light* falling on the analyzing system *is plane polarized* letting

$$\left.\begin{aligned} R' - R &= c \\ r' - r &= n \end{aligned}\right\} \quad \text{and} \qquad (1)$$

he derives the following equations:

$$\cos 2\pi N_1 = \frac{\operatorname{tg} c}{\operatorname{tg} n} \quad \text{or} \quad \operatorname{tg} \pi N_1 = \sqrt{\frac{\sin (n - c)}{\sin (n + c)}}, \qquad (2)$$

and

$$\operatorname{tg} 2\eta = \frac{\sin n \sin 2\pi N_1}{\operatorname{tg} c} = \sqrt{\frac{\sin (n + c) \sin (n - c)}{\sin c}}, \qquad (3)$$

where $2\pi N_1$ is the order of the compensator (in degrees) and

[1] It is well to eliminate circle defects by making the supplementary matches also.

[2] Tuckerman, L. B., University of Nebraska Studies, Vol. IX., No. 2, 1909.

$$\text{tg } 2\eta = -\sin\alpha \text{ tg } 2\pi N,$$

2η being designated as the *effective order* of the half-shade,[1] $2\pi N$ the average order of the two parts, and α the angle between its axis and that of the nicol. Hence having measured c, n and α the orders of half-shade and compensator may be calculated. Usually the *effective order* only of the half-shade is necessary, therefore in the rest of this paper it will be referred to as the order of the half-shade.

If either N_1 or η be known, a convenient form is

$$\left.\begin{aligned} \text{tg } 2\eta &= \frac{\sin(n+c)}{\sin c}\,\text{tg}\,\pi N_1, \\ \text{or} \qquad &= \frac{\sin(n-c)}{\sin c\,\text{tg}\,\pi N_1}. \end{aligned}\right\} \tag{4}$$

When the initial light is plane polarized and the order of the compensator large, that of the half-shade (2η) must also be large in order that the compensator and nicol rotations be of a magnitude such that $2\pi N_1$ may be determined with sufficient accuracy, maximum accuracy being secured when

$$\frac{\sin^2 n - \sin^2 c}{\sin^2 n} = \tfrac{1}{2}\sin^2 2\pi N. \tag{5}$$

The second method (to be used when 2η is small) provides sufficient accuracy when *the initial light* is *elliptically polarized*—the maximum accuracy in this case being obtained when

$$\frac{\sin^2 c - \sin^2 n}{\sin^2 c} = \tfrac{1}{2}\sin^2 2\pi N. \tag{6}$$

In this case ($e_0 \neq 0$) Tuckerman finds that

$$\text{tg}\,\pi N_1 = \sqrt{\frac{\sin(c-n)}{\sin(c+n)}}\,(1 - a\,\text{tg}\,2\eta + \tfrac{1}{2}a^2\,\text{tg}^2\,2\eta - \cdots), \tag{7}$$

and

$$\text{tg}\,2\psi = \sqrt{\frac{\sin(c-n)\sin(c+n)}{\sin n}}\,(1 + \tfrac{1}{2}a^2\,\text{tg}^2\,2\eta - \cdots), \tag{8}$$

[1] The effective order of the half-shade may be varied to a certain extent by changing its azimuth with respect to the analyzing nicol.

where

$$a = \frac{\sin c}{+\sqrt{\sin (c - n) \sin (c + n)}},$$

and

$$e_0 = \operatorname{tg} \psi = \frac{\text{minor axis}}{\text{major axis}}.$$

The series in (7) and (8) are not convergent for small values of e_0. For such cases the proper equations are given in the article mentioned, in which it is also shown that in the case of an uncalibrated compensator the maximum accuracy is obtained in the determination of e_0 when the compensator is a quarter wave plate.

Both of the above methods were used here in the calibration of compensators and half-shades.

Calibration of Compensators and Half-Shades.

First Method.—In this method a 90° mica compensator (no. 1) and a 20° half-shade (no. 2) were used in the elliptic analyzer and calibrated simultaneously by observations taken on plane polarized light. In all calibration measurements the practice followed (unless otherwise stated) was to make but a single observation on R, R', r and r' for numerous points in the spectrum in preference to averaging several observations for relatively few points. The probable value for any wave-length was then obtained from the curve. The values of πN_1 and 2η were calculated from c and n according to equations (1), (2) and (3). Selected values are given in Table I. and all values plotted in Fig. 1 — the order of the half-shade being in all cases increased by 10°.

Table I.

Simultaneous Calibration of Compensator and Half-shade. Compensator No. 1. Half-shade No. 2. e_0=0.

Wave-length ($\mu\mu$).	c	n	Order of Half-shade (2η).	¼ Order of Comp. (πN_1).
660	71.80°	85.70°	17.70°	38.24°
620	71.10	86.90	18.65	40.45
580	69.90	88.67	20.05	43.17
540	68.55	90.95	21.42	46.22
500	66.50	94.01	23.20	49.63
460	63.30	98.80	25.30	53.97

Table II. gives selected values also obtained by the same method for a selenite compensator (no. 2) and the same mica half-shade no. 2. Since the specimen of selenite did not give a good field, the probable error in the results is large. The plot of all values is given in Fig. 1.

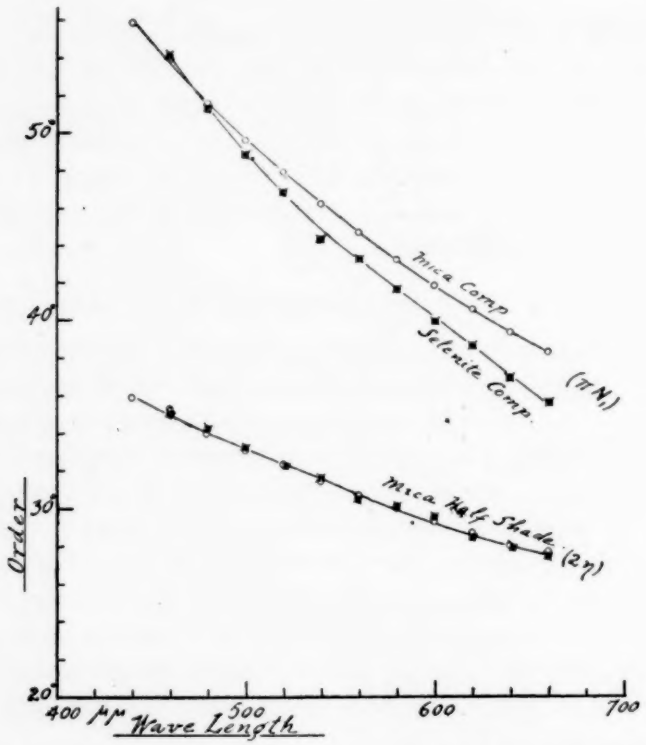

Fig. 1. Calibration of compensators Nos. 1 and 2 and halfshade No. 2.

TABLE II.

Simultaneous Calibration of Compensator and Half-shade. Compensator No. 2. Half-shade No. 2. $e_0=0$.

Wave-length ($\mu\mu$).	Order of Half-shade (2η).	$\frac{1}{2}$ Order of Compensator (πN_1).
660	17.4°	35.6°
640	18.0	36.9
600	19.5	39.9
560	20.5	43.2
520	22.1	46.8
480	24.1	51.3
460	25.0	54.1

This same method which serves very well for determining the order of thin half-shades by the use of a thick compensator was used to obtain the order of a thin half-shade (no. 1). Several observations made on one color indicated that the probable error in a single determination of R', R, etc., was not over .02°. Table III. gives the values obtained for half-shade no. 1 by compensator no. 1. The calculated values for 2η were obtained by taking the observed order for 590 $\mu\mu$ and assuming the dispersion in this to be the same as that obtained for compensator no. 1 (see Fig. 2).

TABLE III.

Calibration of Half-shade. Compensator No. 1. Half-shade No. 1. $e_0 = 0$.

Wave-length ($\mu\mu$).	Comp. Settings.	Nicol Settings.	c	n	Order of Half-shade (2η).	
					Obs.	Calc.
640	88.16°	93.03°	89.10°	89.81°	.88°	.87°
	359.06	3.22				
590	88.14	93.06	89.06	89.91	.94	—
	359.08	3.15				
540	88.12	93.13	89.01	90.02	.99	1.02
	359.11	3.11				
490	88.09	93.21	88.85	90.19	1.13	1.12
	359.24	3.02				

Table IV. gives the results obtained in the same way for another thin half-shade (no. 3). Greater accuracy was desired in these results in order to calibrate a thin compensator by comparison, hence they were obtained from the average of several observations —the probable error in 2η being not greater than .01°. Two compensators were used. No. 1 for 600 and 590 $\mu\mu$ and incidentally no. 2 for 580.

TABLE IV.

Calibration of Half-shade. Half-shade No. 3. $e_0 = 0$.

Comp.	Wave-length ($\mu\mu$).	c	n	Order of Half-shade (2η).
No. 1	600	88.24°	89.76°	1.745°
	590	88.22	89.80	1.770
No. 2	580	92.25	91.36	1.795

If the value of 2η be properly chosen and accurately known, a thin compensator such as used in the Brace instrument may be calibrated with sufficient accuracy by comparison. This method is identical with the above excepting that equations (4) are applicable in place of (2). Table V. gives the results obtained for compensator no. 3 by comparison with half-shade no. 3, and applying the *first* equation, since the factor $(c-n)$ which enters in the *second* is in this case too small to give the desired accuracy. The results are the average of several observations and the order accurate to .05°.

TABLE V.

Calibration of Compensator by Comparison. Compensator No. 3. Half-shade No. 3. $e_0=0$.

Wave-length ($\mu\mu$).	c	n	$(c+n)$	(2η)	$(2\pi N_1)$
600	106.22°	106.14°	212.38°	1.745°	6.25°
580	106.20	106.12	212.32	1.770	6.44

Second Method.—As before stated, if the elliptic analyzer contain a thick compensator and thin half-shade, the calibration of the compensator can be accomplished if the initial light be transformed from plane to elliptically polarized light by some means, such as introducing a doubly refracting plate between the polarizer and analyzing system, the observations being taken as before. The initial ellipticity (e_0) and the order of the compensator ($2\pi N_1$) are then calculated by applying equations (7) and (8).[1] Compensator no. 1 was recalibrated by this method, half-shade no. 1 being used. Selected results are given in Table VI. and all plotted in Fig. 2.

TABLE VI.

Simultaneous Calibration of Compensator and Measurement of Ellipticity. Compensator No. 1. Half-shade No. 1. $e_0 \neq 0$.

Wave-length ($\mu\mu$).	πN_1	$\frac{1}{2}$ arc tg e_0	Wave-length ($\mu\mu$).	πN_1	$\frac{1}{2}$ arc tg e_0
700	36.80°	29.33°	540	46.50°	30.30°
670	37.88	29.73	500	49.85	29.20
640	39.60	30.00	470	52.92	28.02
600	42.07	30.43	440	55.68	26.76
570	44.12	30.55	400	59.96	24.25

[1] Since in these equations 2η enters only to produce small corrections, its values were obtained from Table III. and the uncorrected dispersion curve of the compensator as obtained from (7) assuming $\eta=0$.

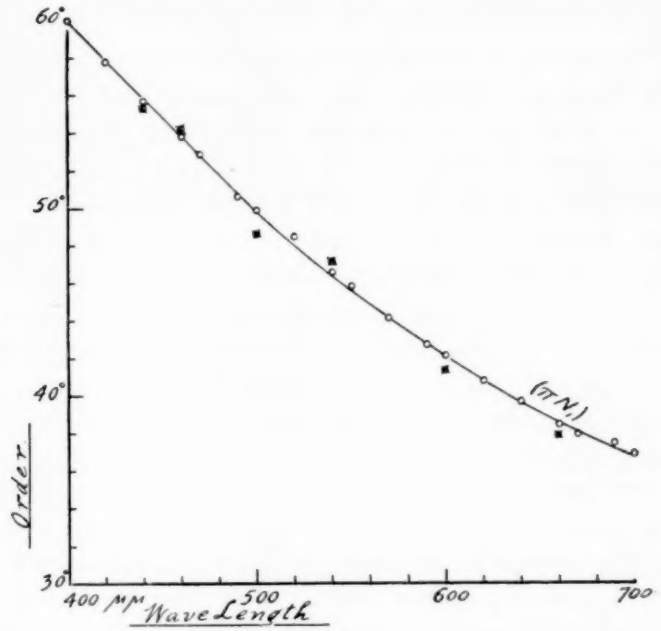

Fig. 2. Dispersion of mica compensator No. 1. (Crossed circles adapted from McComb's calibration of a 6° compensator.)

The accuracy obtained in the above calibrations shows that with the proper refinements these methods may be used to determine the order of half-shade and compensator of the modified Stokes analyzing system, and also for the analyzing parts of any other elliptic half-shade instrument now in use. The differential dispersion of any doubly refracting plate whatever may also be easily and accurately determined by calibrating it as a compensator of this modified system. These results for the differential dispersion of a mica compensator agree (as shown in Fig. 2) with McComb's which were obtained from a plate $\pi N_1 = 3°$ for 600 $\mu\mu$, by an entirely different method.[1]

Since however the value of e_0 may be calculated without that of $2\pi N_1$ being known, the above calibrations for the compensators were used in the following work on metallic reflection only as a check on the results.

[1] McComb, H. E., PHYS. REV., Vol. XXIX., p. 525, 1909.

II. Optical Constants of Metals.

The dispersion of metals has been studied by means of the two general methods, reflection and transmission. The latter comprises the prism and interferometer methods for obtaining the refractive indices and the use of the spectro-photometer for the absorption coefficients. Because of the difficulties involved in these, the reflection method has been used to a greater extent. This latter method of obtaining the metallic constants is represented by the work of many investigators, notably that of Drude.[1] He carried out a very careful investigation as to the effect of surface conditions, but unfortunately determined the constants for but two points in the spectrum from which no adequate idea of the dispersion throughout the spectrum is to be obtained. A number of other investigations have been made extending over a much wider range, notable among them being the work of Minor.[2] By the use of Voigt's[3] ultra-violet method he obtained the dispersion from 230 to 630 $\mu\mu$ for silver, steel, copper and cobalt. His curves for the refractive indices and absorption coefficients show strikingly the characteristic effect of transmission regions, especially with silver in the ultra-violet and copper in the yellow. With copper the color suggests that the effect is to be found in the visible spectrum. One would expect, as shown below, that gold also would possess similar peculiarities in this region, and perhaps other metals, provided the observations be properly distributed.

Methods and Equations.

The method and apparatus described in Part I. were used for measuring the ellipticity produced in plane polarized light by reflection from metallic mirrors and from these measurements the optical constants of the metals were calculated.

M'Connel's method[4] of an auxiliary nicol mounted upon the spectrometer table was used for adjusting the polarizer at 45° to the plane of incidence. The metal mirrors were mounted on the spec-

[1] Drude, P., Wied. Ann., Vol. 39, p. 481, 1890.
[2] Minor, R. S., Ann. d. Physik, Vol. 10, p. 581, 1903.
[3] Voigt, W., Physik. Zeitschr., Vol. 2, p. 303, 1901.
[4] M'Connel, J. C., Phil. Mag. (5), Vol. 19, p. 317, 1885.

trometer table by means of a universal adjustment, the adjustments made and the angle of incidence (θ) determined in the customary way. Using compensator no. 1 and half-shade no. 3, the settings for the four different matches were made in every case.

With the *plane of vibration* (electrical) of the incident light as the *plane of reference*, the azimuth of the major axis of the reflected ellipse is given by the expression

$$2\gamma' = (R' + R) - (R_0' + R_0),$$

R' and R being the compensator settings on the reflected, while R_0' and R_0 are the corresponding settings on the incident light. This follows from the fact that the position of the major axis is always given by the expression $[(R' + R)/2 - \pi/4]$ when R' and R are complementary settings[1] with respect to the major axis as the axis of reference.

With the *plane of incidence* as the *plane of reference*, the azimuth of the major axis of the reflected ellipse is given by the corresponding expression

$$2\gamma = (R' + R) - (R_0' + R_0) - 2\varphi', \tag{9}$$

φ' being the azimuth of the incident vibration.

It is evident that for the incident light cot φ' gives the ratio of the component amplitudes in and perpendicular to the plane of incidence; likewise cot φ the corresponding ratio for the reflected light, φ being the azimuth of the restored vibration. If the difference in phase between these components of the reflected light be designated by δ the optical constants for the angle of incidence θ are given by the equations:

$$\nu_\theta = -\frac{\sin\theta \operatorname{tg}\theta(\cos 2\varphi + \cos 2\varphi')}{1 + \cos 2\varphi \cos 2\varphi' - \sin 2\varphi \sin 2\varphi' \cos\delta},$$

$$\nu_\theta\kappa_\theta = \frac{\sin\theta \operatorname{tg}\theta \sin 2\varphi \sin 2\varphi' \sin\delta}{1 + \cos 2\varphi \cos 2\varphi' - \sin 2\varphi \sin 2\varphi' \cos\delta},$$

$$\kappa_\theta = -\frac{\sin 2\varphi \sin 2\varphi'}{\cos 2\varphi + \cos 2\varphi'} \sin\delta;$$

ν_θ being the refractive index, κ_θ the extinction coefficient per wave-

[1] A complementary pair of settings satisfies the condition that $\cos c/\cos 2\pi N_1 > 0$. The condition stated by Tuckerman (loc. cit., p. 21) is incorrect.

length in the medium considered, and $v_\theta \kappa_\theta$ the same per wave-length *in vacuo* — all for the angle of incidence θ.

For $\varphi' = 45°$ as customarily used, it follows, since from the well known equations $\sin 2\psi = \sin 2\varphi \sin \delta$ and $\cos 2\psi \sin 2\gamma = \sin 2\varphi \cos \delta$, that

$$\left.\begin{aligned} v_\theta &= -\frac{\sin\theta \operatorname{tg}\theta \cos 2\psi \cos 2\gamma}{1 - \cos 2\psi \sin 2\gamma}, \\ v_\theta \kappa_\theta &= \frac{\sin\theta \operatorname{tg}\theta \sin 2\psi}{1 - \cos 2\psi \sin 2\gamma}, \\ \kappa_\theta &= -\frac{\operatorname{tg} 2\psi}{\cos 2\gamma}. \end{aligned}\right\} \tag{10}$$

The constants for perpendicular incidence ($\theta = 0$) were obtained from the following approximation formulæ which for all cases were accurate to within 0.2 per cent. Omitting terms of the second order they reduce to the equations as used by Drude.

$$\left.\begin{aligned} v_0 &= v_\theta \left[1 + \frac{\sin^2\theta}{2v_\theta^2(1+\kappa_\theta^2)} + \tfrac{1}{2}\left(\frac{v_\theta^2\kappa_\theta^2}{v_\theta^2(1+\kappa_\theta^2)} - \tfrac{1}{4}\right)\frac{\sin^4\theta}{v_\theta^4(1+\kappa_\theta^2)^2} - \cdots\right],^{1} \\ v_0\kappa_0 &= v_\theta\kappa_\theta \left[1 - \frac{\sin^2\theta}{2v_\theta^2(1+\kappa_\theta^2)} + \tfrac{1}{2}\left(\frac{v_\theta^2}{v_\theta^2(1+\kappa_\theta^2)} - \tfrac{1}{4}\right)\frac{\sin^4\theta}{v_\theta^4(1+\kappa_\theta^2)^2} - \cdots\right], \\ \kappa_0 &= \kappa_\theta \left[1 - \frac{\sin^2\theta}{v_\theta^2(1+\kappa_\theta^2)} + \frac{v_\theta^2}{v_\theta^2(1+\kappa_\theta^2)} \cdot \frac{\sin^4\theta}{v_\theta^4(1+\kappa_\theta^2)^2} - \cdots\right] \end{aligned}\right\} \tag{11}$$

The principle incidence ($\bar{\theta}$) and azimuth ($\bar{\psi}$) were also calculated by the following approximation formulæ which are easily obtained from the rigid formulæ.

$$\left.\begin{aligned} \sin\bar{\theta}\operatorname{tg}\bar{\theta} &= \sqrt{v_0^2(1+\kappa_0^2)}\left[1 - \tfrac{1}{2}\frac{v_0^2(1-\kappa_0^2)}{v_0^2(1+\kappa_0^2)} \cdot \frac{\sin^2\bar{\theta}}{v_0^2(1+\kappa_0^2)} + \tfrac{1}{8}\left(2 - 3\left(\frac{1-\kappa_0^2}{1+\kappa_0^2}\right)^2\right)\frac{\sin^4\bar{\theta}}{v_0^4(1+\kappa_0^2)^2} - \cdots\right], \\ \operatorname{tg} 2\bar{\psi} &= \kappa_0\left[1 + \frac{\sin^2\bar{\theta}}{v_0^2(1+\kappa_0^2)} + \frac{v_0^2}{v_0^2(1+\kappa_0^2)} \cdot \frac{\sin^4\bar{\theta}}{v_0^4(1+\kappa_0^2)^2} + \cdots\right]. \end{aligned}\right\} \tag{12}$$

[1] The retention of v_θ in both numerator and denominator in these equations renders the computation more convenient.

The reflection coefficient (J) was obtained from the equation

$$J = \frac{\nu_0^2(1 + \kappa_0^2) + 1 - 2\nu_0}{\nu_0^2(1 + \kappa_0^2) + 1 + 2\nu_0}. \qquad (13)$$

Effect of Surface Condition.

Surface films and impurities picked up during the grinding and polishing process are the chief sources of error in obtaining the optical constants by reflection. Since there is no evidence of their presence other than their effect on the magnitude of the constants, and since the different types of films produce different effects, and even the same type may have a different effect for every metal, it is necessary that a large mass of data be collected before it is possible to determine when a good surface has been obtained and even then there is doubt unless the general results of other investigators be taken as a criterion.

Because of the impossibility of producing a good reflecting surface without great risk of these disturbing effects, there has been considerable investigation concerning them. The results indicate that in general the films formed during the polishing process, whether arising from impurities or other causes, tend to increase the value of all the constants, especially the absorption and reflection coefficients. The type of film, however, which is formed when the surface is exposed to the air, usually has the opposite effect. Considerable study of this latter effect was made in the present investigation and it was observed that, with all metals, films were formed in a comparatively short time. Gold and nickel were the least affected of those investigated, while copper and zinc surfaces underwent an appreciable change with only an hour's exposure even in a very dry atmosphere. Unless care is taken, oil and moisture films form during the polishing process and also by exposure. The effect of these more or less transparent films, if they are very thin and have not attacked the surface, is comparatively small, as was found by testing a steel mirror before it was thoroughly cleansed.

A matt surface or incomplete polish also reduces the value of the constants, the greatest change again being in κ_0 and J. That a granular or crystalline structure is productive of considerable

error is revealed by changing the angle of incidence. This effect was most noticeable in the case of a cathode film of cobalt which when inspected by a low power microscope appeared to have a smooth glassy surface, but under a high power showed a very fine granular structure. The magnitude of the fluctuation in ν_0 and $\nu_0\kappa_0$ for a change of 30° in the angle of incidence was in this case about 5 per cent. though when carefully polished most fused metals do not show greater than 0.5 per cent. fluctuations.

Because of the large effect exercised by these surface conditions it is difficult to obtain results that will agree closely even when the changes in the method of polishing are slight. It must also be concluded from the large variation in the results produced by entirely different methods that it is impossible to burnish or polish a metal without mixing the surface with impurities which enter so deeply that no "purifying" process whatever will remove them. Furthermore, the oxide films for which such processes are chiefly intended, are in all probability formed after the surface has been fully prepared. One is inclined to question also whether burnishing and even polishing does not change the original structure of the metal sufficiently to cause a change in the constants.

The various investigators have used widely different methods in producing and treating their mirrors, and the results obtained have also differed widely. It appears, therefore, that the true value of the metallic constants cannot be determined to better than a probable accuracy of two to three per cent. It is important to note, though, that the *form of the dispersion curve* seems to depend much less upon the surface conditions and hence is more reliable.

In view of the above facts, several methods[1] of preparing mirrors were tried while endeavoring to produce a good reflecting surface with the least chance of contamination. The best results were obtained by first making the surface as smooth and plane as possible by the use of a fine clean file, then finally grinding with very fine emery dusted over soft dry paper laid upon a plane piece of glass. The mirror was then polished with rouge on kid or sometimes in the case of the softer metals on clean kid.

[1] Several kinds of silver polish were tried and very brilliant mirrors obtained, but it was feared that the moisture or oils in them would produce disturbing surface films.

In this way, very good mirrors were obtained which were quite free from scratches. Difficulty was encountered in securing plane surfaces only when polished too long upon the leather. All mirrors were uniform over the whole surface though an area of 3×4 mm. only was needed.

Experimental Results.

Nickel.[1]—The mirrors were made of pure rolled nickel (from Eimer & Amend) and were easily given a brilliant reflecting surface. Several series of observations were taken on different mirrors, these showing a large variation in the value of the constants. Those given in Table VII. for selected wave-lengths were obtained under

Table VII.

Nickel.[2] $\theta = 65°$.

Wave-length ($\mu\mu$).	ν_0	κ_0	$\nu_0\kappa_0$	$\bar{\theta}$	$\bar{\psi}$	J
700	2.025	1.965	3.980	77.83°	32.05°	.676
660	1.945	1.975	3.840	77.42	32.15	.668
620	1.820	1.985	3.610	76.70	32.30	.653
580	1.725	1.975	3.405	76.00	32.30	.637
540	1.625	1.965	3.200	75.25	32.35	.621
500	1.540	1.930	2.975	74.33	32.25	.597
460	1.460	1.885	2.750	73.37	32.10	.572
420	1.415	1.790	2.530	72.33	31.70	.537

[1] Kundt, A., Wied Ann., Vol. 34, p. 469, 1888. DuBois and Rubens, Ann. d. Physik, Vol. 8, p. 17, 1902.

[2] For the sake of brevity, the following example only is given to show the magnitude of the various observed quantities and the method of calculating the constants in this table:

Constants of the instrument:

$R_0' + R_0 = 267.60°$ $\quad \phi' = 45°$ $\quad 2\eta = 1.93°$

Observations for $\lambda = 540\,\mu\mu$, $\theta = 65°$:

$R' = 287.88°$ $\quad r' = 334.25°$ $\quad R' + R = 480.03°$

$R = 192.15$ $\quad r = 292.99$ $\quad c + n = 137.08$

$c = 95.73$ $\quad n = 41.35$ $\quad c - n = 54.38$

First calculations (from equations (7), (8), (9) and (10)):

$\pi N_1 = 46.25°$ $\quad 2\gamma = 122.43°$ $\quad 2\psi = 48.40°$

$\nu_\theta = 1.575$ $\quad \kappa_\theta = 2.090$ $\quad \nu_\theta\kappa_\theta = 3.300$

Final results (from equations (11), (12) and (13)):

$\nu_0 = 1.625$ $\quad \kappa_0 = 1.965$ $\quad \nu_0\kappa_0 = 3.200$

$\bar{\theta} = 75.25°$ $\quad \bar{\psi} = 32.35°$ $\quad J = 0.621$

what were considered the best surface conditions, observations having been taken within three hours after the mirror had been carefully polished. In general the other mirrors gave lower results for ν_0 and $\nu_0\kappa_0$, though κ_0 was usually larger. The lowest values for ν_0 were obtained from a highly polished mirror two weeks after it had been prepared. For this, ν_0 was found to be 1.63 ($\lambda = 580$) which is

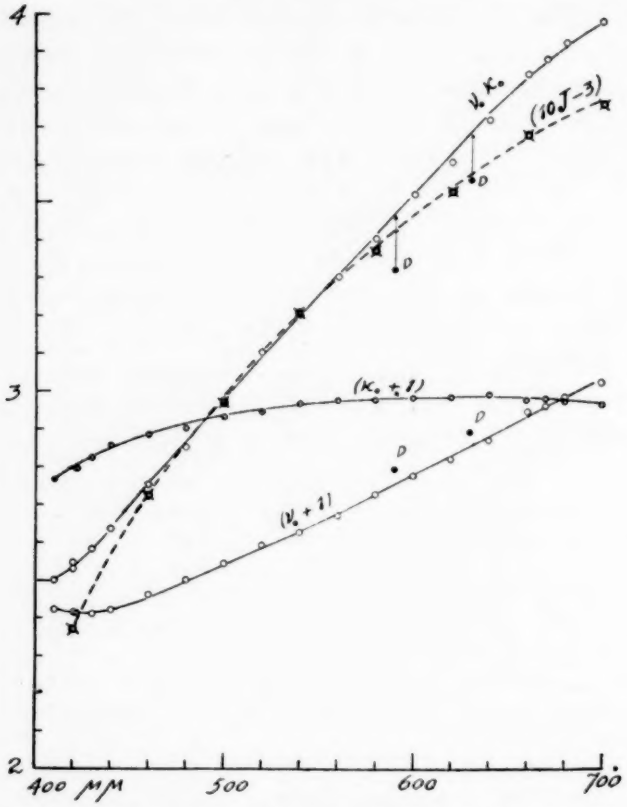

Fig. 3. *Nickel.* Drude's values for ν_0 and $\nu_0\kappa_0$ indicated by *D*.

about 6 per cent. lower than the value given in the table, while κ_0 was 2.05 or 3 per cent. higher. It is noteworthy that the slope and form of the curves through the spectrum were, however, the same in the various cases. Neither was there any great variation observed on changing the angle of incidence, there being in the case of one of the poorer mirrors only about 0.5 per cent. deviation between 55° and 75°.

On the curves (Fig. 3) are indicated all the values obtained from the chosen series of observations. To bring the various curves within the same plot their ordinates were changed as indicated.

The refractive indices (ν_0) are about 2 per cent. lower than Drude's (indicated by D) while the absorption coefficients (κ_0) are 6 per cent. higher. The curves lie parallel to the two values he gives showing the same dispersion in that region of the spectrum. The values for the principal incidence ($\bar{\theta}$) and azimuth ($\bar{\psi}$) are higher than Drude's, but agree closely with those of Quincke.[1] Excepting those at the blue end, which are about 5 per cent. lower, the reflection coefficients (J) are about the same magnitude as those given by Hagen and Rubens.[2]

Some evidence of a transmission region in the extreme violet is shown by both the refractive index (ν_0) and the absorption coefficient (κ_0) curves.

Cobalt.[3]—The cobalt mirrors were made of the pure metal. They showed about the same effects of polish as did those of nickel. The results given in Table VIII. and Fig. 4 were obtained from three series of observations on the same mirror but at different angles of incidence. The refractive index is 3 per cent. higher and the absorption coefficient about 4 per cent. lower ($\lambda = 590$) than given by Drude. This divergence is less than Minor's original observations show but he corrected his curves to agree with the values given by Drude, the dispersion being the same.

TABLE VIII.

Cobalt.

Wave-length ($\mu\mu$).	ν_0	κ_0	$\nu_0\kappa_0$	$\bar{\theta}$	$\bar{\psi}$	J
680	2.500	1.775	4.440	79.20°	30.75°	.687
640	2.340	1.815	4.245	78.70	31.05	.679
600	2.210	1.825	4.030	78.15	31.15	.667
560	2.090	1.825	3.810	77.50	31.20	.653
520	1.975	1.830	3.615	76.90	31.35	.640
480	1.855	1.835	3.405	76.15	31.45	.623
440	1.705	1.870	3.190	75.30	31.75	.610
420	1.680	1.830	3.070	74.90	31.60	.596

[1] Quincke, G., Pogg. Ann. Jubelbd., p. 345, 1874.
[2] Hagen & Rubens, Ann. d. Physik, Vol. 8, p. 17, 1902.
[3] Drude, P., Wied. Ann., Vol. 42, p. 189, 1891.

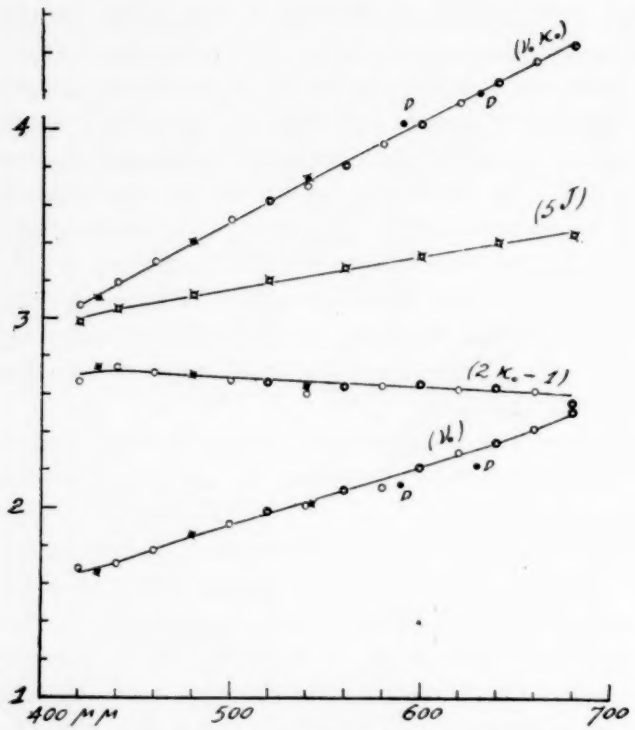

Fig. 4. *Cobalt.* Rings: $\theta = 75°$. Circles: $\theta = 70°$. Crossed circles: $\theta = 60°$. Drude's values indicated by *D*.

Steel.—Two mirrors made from file steel which gave a good reflecting surface were tested. Different methods of polishing gave

TABLE IX.

Steel. $\theta = 70°$.

Wave-length ($\mu\mu$).	n_0	κ_0	$n_0\kappa_0$	$\bar{\theta}$	$\bar{\psi}$	J
700	2.73	1.270	3.47	77.60	26.60	.580
690	2.72	1.270	3.46	77.55	26.60	.579
660	2.67	1.280	3.41	77.40	26.65	.575
620	2.58	1.280	3.30	77.05	26.70	.568
580	2.46	1.315	3.24	76.70	27.15	.564
560	2.40	1.340	3.21	76.65	27.40	.561
520	2.26	1.380	3.12	76.05	27.90	.556
480	2.07	1.455	3.01	75.40	28.65	.552
440	1.86	1.545	2.87	74.55	29.55	.547
420	1.79	1.575	2.82	74.15	29.80	.545

a considerable range of values for the constants. The series of observations chosen as representative were taken on a rouge polished surface. One with its surface prepared a year before gave for ν_0 the value 2.18 (580 $\mu\mu$) which is 10 per cent. lower than Drude's indicate; and for κ_0 the value 1.43 which is about 4 per cent. higher than his. Repolished this mirror gave the results in Table IX. and Fig. 5.

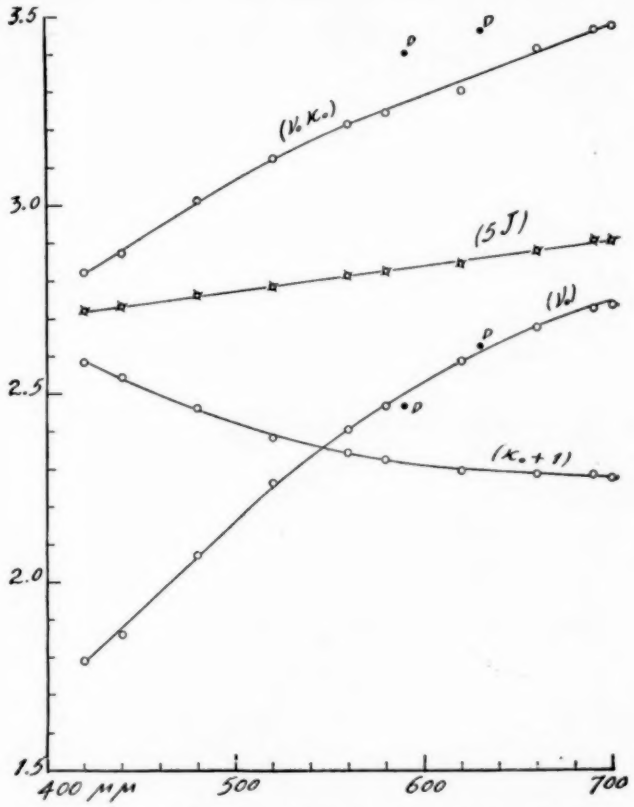

Fig. 5. *Steel.* Curves as obtained under "best" conditions.

Copper.[1] — The mirrors finally used were of ordinary commercially "pure" copper as the chemically pure obtained for the purpose was too full of flaws to give a satisfactory reflecting surface. On account of the rapid tarnishing it was found necessary to take the

[1] Shea, D., Ann. d. Physik, Vol. 47, p. 202, 1892.

observations immediately after the mirror had been polished. The time effect reduces the value of all the constants much more rapidly at first than after several hours. This is probably the reason why Minor did not note an effect of this kind. Table X. shows the change observed in the value of the constants with time. In Fig. 6 are given curves which show the great variation arising from different treatment in polishing. Table XI. and Fig. 7 give values obtained under what was considered the best conditions, although curves b and b' in Fig. 6 represent values which agree much more

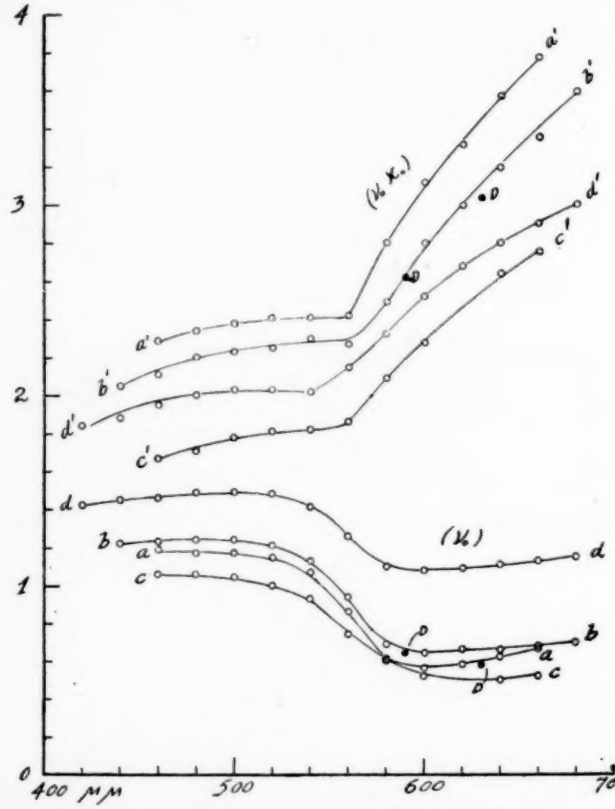

Fig. 6. *Copper.* Curves a and a' obtained under "best" conditions; b and b' from a mirror with incomplete polish; c and c' from one oxidized by heating; d and d' from one oxidized by exposure to moist air.

closely with Drude's. Those chosen differ considerably from his, ν_0 being over 9 per cent. lower (590 $\mu\mu$), while κ_0 is about 25 per cent.

TABLE X.

Copper—Time Effect.

Wave-length ($\mu\mu$).	Taken Immediately.		After Three Hours.		After Twenty Hours.	
	ν_0	$\nu_0\kappa_0$	ν_0	$\nu_0\kappa_0$	ν_0	$\nu_0\kappa_0$
640	.615	3.580	.625	3.490	.605	3.395
600	.565	3.115	.555	3.040	.535	2.970
580	.605	2.720	.605	2.685	.570	2.620
540	1.070	2.410	1.050	2.395	1.025	2.345
500	1.170	2.380	1.170	2.370	1.170	2.350
460	1.180	2.290	1.175	2.250	1.155	2.210

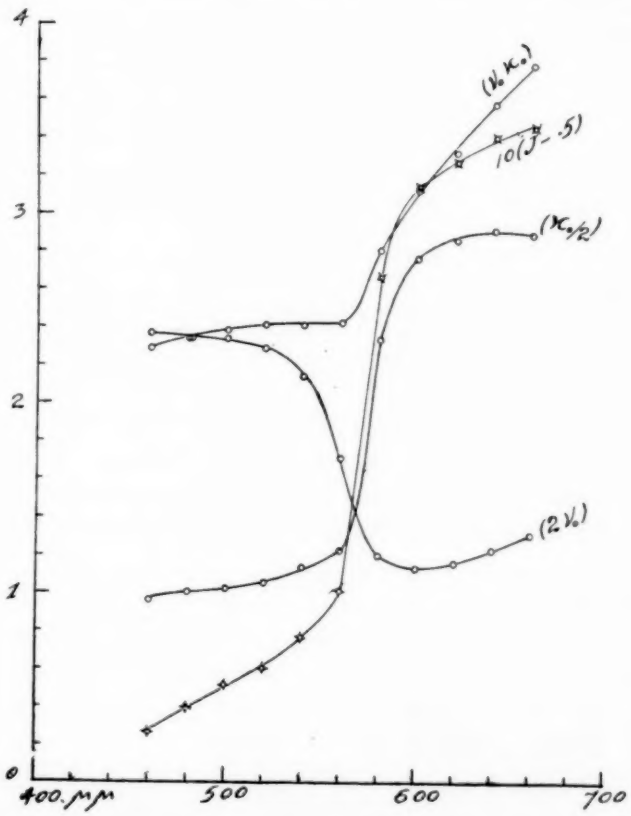

Fig. 7. *Copper.* Curves as obtained under "best" conditions.

higher. The form of the dispersion appears to be different also, his values suggesting a continuous slope toward the red, while the

curves in Fig. 6 show in every case a definite rise from about 600 to 700 $\mu\mu$. The results of Minor in this region apparently agree with those of Drude though not necessarily since he calculated his value for 630 $\mu\mu$ from the dispersion given by Drude. All curves show the characteristics of a transmission band at about 560 $\mu\mu$.

Because of the large values of ν_0 and $\nu_0\kappa_0$ the calculated values for $\bar{\theta}$, $\bar{\psi}$ and J are also larger than usually given by other investigators.

TABLE XI.

Copper. $\theta = 60°$.

Wave-length ($\mu\mu$).	ν_0	κ_0	$\nu_0\kappa_0$	$\bar{\theta}$	$\bar{\psi}$	J
660	.655	5.770	3.780	76.20°	40.40°	.846
640	.615	5.820	3.580	75.55	40.45	.841
620	.580	5.720	3.320	74.60	40.40	.828
600	.565	5.510	3.115	73.80	40.30	.814
580	.600	4.660	2.795	72.35	39.55	.767
560	.855	2.830	2.420	70.75	36.35	.601
540	1.070	2.250	2.410	71.05	34.15	.576
520	1.145	2.105	2.410	71.15	33.60	.560
500	1.170	2.030	2.375	71.05	33.20	.551
480	1.170	1.995	2.335	70.80	33.10	.539
460	1.185	1.930	2.285	70.60	32.75	.526

Gold.[1]—Three series of observations were taken on a mirror of "pure" gold procured from Sargent & Co., Chicago. The values in the first series (Fig. 8, curves b and b') agree closely with those of Drude for 18-*carat* gold, but in the second (represented by the circles on curves a and a' in Fig. 8) taken after the mirror had been reground and polished, the results are about the magnitude of those obtained by other investigators for *pure* gold. It is remarkable to note that while the product $\nu_0\kappa_0$ is the same for both throughout the spectrum, the separate values of ν_0 and κ_0 are alike in the blue only and diverge widely in the red. The third series (represented by dots on curves a and a') was taken after the mirror had been again thoroughly repolished with rouge. The values in this are practically identical with those of the second series. Table XII. and Fig. 9 give the average value of the second and third series.

[1] Shea, D., loc. cit.

Since the gold mirrors change very little by exposure the variation shown is probably to be ascribed to the polishing, especially since the observations in all three cases were taken within a few hours

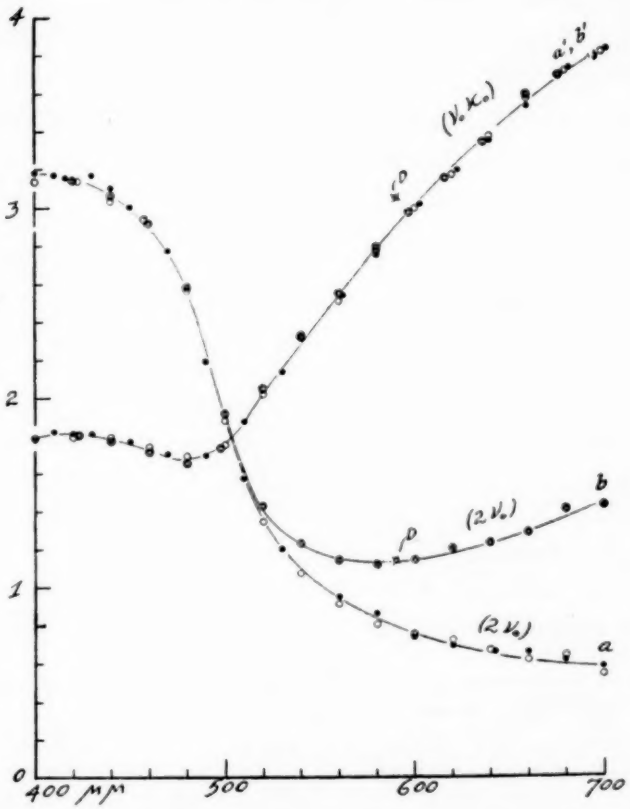

Fig. 8. *Gold.* Curves *b* and *b'* obtained from first series of observations; circles on *a* and *a'* from second series; dots on *a* and *a'* from third. Drude's values for *18 carat* gold as indicated.

after the mirror had been prepared. In view of these results it is probable that the comparatively low value obtained by Drude for the absorption coefficient of 18-carat gold was due to the condition of the surface, since it would be surprising to find the absorption of an alloy of gold, copper and silver less even than the average of gold and copper—the value for silver being greater than that for gold.

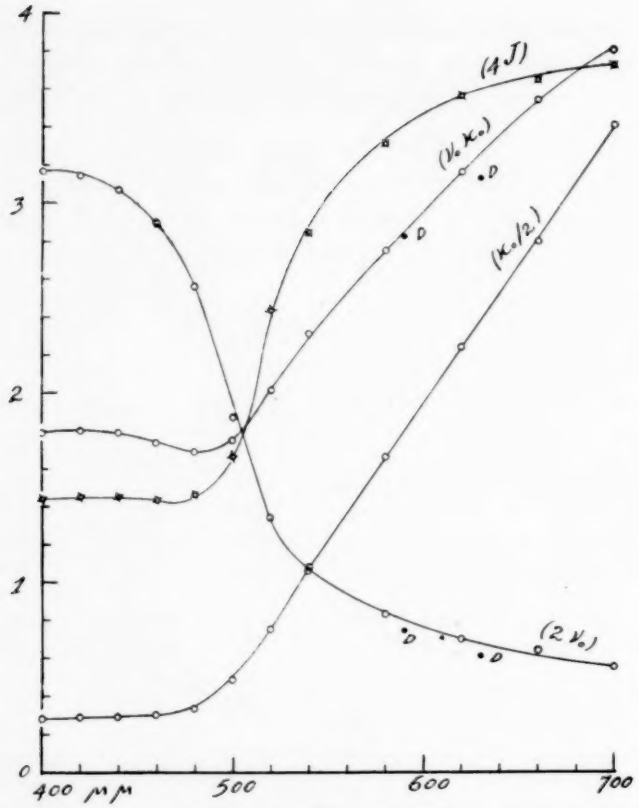

Fig. 9. *Gold.* Curves as obtained from the mean of the second and third series of observations. Drude's values for *pure* gold as indicated.

TABLE XII.

Gold. $\theta = 60°$.

Wave-length (μμ).	ν_0	κ_0	$\nu_0\kappa_0$	$\bar{\theta}$	$\bar{\psi}$	J
700	0.280	13.60	3.800	76.10°	43.05°	.930
660	0.320	11.20	3.540	75.30	42.65	.910
620	0.350	8.95	3.160	73.90	42.10	.889
580	0.415	6.65	2.750	72.05	41.15	.827
540	0.535	4.32	2.305	69.70	39.35	.710
520	0.670	2.99	2.010	67.90	37.15	.608
500	0.935	1.870	1.750	66.55	33.15	.415
480	1.280	1.315	1.685	67.15	28.85	.364
460	1.450	1.200	1.740	68.20	27.25	.358
440	1.535	1.165	1.790	68.80	26.80	.362
420	1.570	1.145	1.800	68.95	26.55	.362
400	1.580	1.130	1.785	68.90	26.35	.360

As suggested by the color of the reflected light, the dispersion curves for gold show the effect of a transmission band, the center of which lies in the neighborhood of 490 $\mu\mu$. The form of the curves is much the same as those of copper, in which the transmission band is near 560 $\mu\mu$.

Résumé.

An instrument which combines the Stokes and the half-shade principles for measuring ellipticity has been successfully used. It has been shown that this instrument will measure the order of thin doubly refracting plates, such as used in the Brace elliptic analyzer to 0.01°, which is an error of less than one per cent. for the thinnest half-shade so far advantageously employed. It will also calibrate a quarter-wave plate to at least 0.05°, an accuracy of about 0.05 per cent.; or, it will measure an ellipticity of 0.02 to at least one per cent., and an ellipticity of approximately unity to 0.05 per cent. The introduction of the elliptic half-shade enables it further to measure circularly polarized light—where Stokes's analyzer fails.

This method has been applied to the investigation of the optical constants of metals by reflection and determinations have been made throughout the visible spectrum. The maximum sensibility was not attempted here since the variation arising from surface conditions rendered such accuracy useless.

With all the metals studied observations were taken at sufficiently frequent intervals to determine the direction of the dispersion curve. This dispersion is found to agree in general with that of other investigators though the magnitude of the constants chosen as representative is often different.

Of especial interest are the results from copper and gold, showing the characteristics of a transmission band in the visible spectrum, the existence of which for copper, Minor has already shown.

In conclusion the writer wishes to thank Professors Skinner and Tuckerman for the aid given him.

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THE SECOND POSTULATE OF RELATIVITY.

BY RICHARD C. TOLMAN.

IN a recent article by Lewis and Tolman[1] a non-analytical method was developed for obtaining the more important conclusions which can be drawn from the principle of relativity. Our reasoning was based *only* upon the first and second postulates of relativity, and those fundamental conservation laws of mass, energy and momentum which science has never in a single instance been forced to abandon. Since the method of attack avoided any use of involved mathematical analysis, restricting itself to the simplest processes of logical reasoning, and, further, made no use of the assumptions of electromagnetic theory, it may be concluded that the unexpected nature of the results of the theory of relativity is due to something unusual in the two postulates of relativity themselves.

No objections have ever been made to the first postulate of relativity, as stated in its original form by Newton, that it is impossible to measure or detect absolute translatory motion through space. In the development of the theory of relativity, this postulate has been modified to include the impossibility of detecting translatory motion through any ether or medium which might be assumed to pervade space. In support of this principle is the general fact that no "ether drift" has ever been detected, but,

[1] Lewis and Tolman, Proc. Amer. Acad., 44, 711–724, 1909; Phil. Mag., 18, 510–23, 1909. A novel method of proof was adopted in this article which consisted in the consideration of certain experiments which might be performed by two observers situated on similar systems which are in relative motion. The reasoning was based on the supposition that the results obtained in such experiments would not contradict either of the two postulates of relativity nor the conservation laws of mass, energy and momentum. These supposed experiments are analogous to the cyclical processes used in thermodynamic proofs, and bear the same relation to the analytical method used by Einstein in his treatment of the theory of relativity, as the "cyclical process" method bears to the more elegant considerations of the analytically inclined thermodynamist.

especially, the conclusive experiments of Michelson and Morley, and Trouton and Noble, in which, a motion through the ether, of the earth in its path around the sun would certainly have been detected. For the purposes of this article we shall consider that the first postulate of relativity needs no further proof.

It is Einstein (to whom, indeed we owe the development of relativity along its present broad lines) who first stated the second postulate of relativity in a general form, namely, that the velocity of light in free space appears the same to all observers, regardless of the relative motion of the source of light and the observer. This is the assumption which has forced the theory of relativity to its

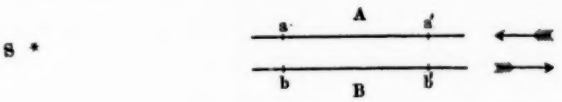

Fig. 1.

strange conclusions, and it is for its further consideration that this paper is designed.

A simple example will make the extraordinary nature of the second postulate evident.

S is a source of light and A and B two moving systems. A is moving towards the source S, and B away from it. Observers on the systems mark off equal distances aa' and bb' along the path of the light and determine the time taken for light to pass from a to a' and b to b' respectively. Contrary to what seem the simple conclusions of common sense, the second postulate requires that the time taken for the light to pass from a to a' shall measure the same as the time for the light to go from b to b'. Such a consideration makes the path obvious by which the theory of relativity has been led to strange conclusions as to the units of length and time in a moving system.

The second postulate of relativity is obtained by a combination of the first postulate with a principle which has long been familiar in the theory of light. This principle states that the velocity of light is unaffected by a motion of the emitting source, in other words, that the velocity with which light travels past any observer

is not increased by a motion of the source of light towards the observer. The first postulate of relativity adds the idea that a motion of the source of light towards the observer is identical with a motion of the observer towards the source. The second postulate of relativity is seen to be merely the combination of these two principles, since it states that the velocity of light in free space appears the same to all observers regardless *both* of the motion of the source of light and of the observer.[1] Since the first postulate of relativity has already been considered as sufficiently proved, we shall proceed at once to present certain evidence in favor of the assumption that the velocity of light is independent of the motion of the source. In the latter part of the paper, we shall also consider an entirely independent proof of the second postulate based on the Kaufmann-Bucherer experiment.

The principle that the velocity of light is independent of the velocity of its source has hitherto lacked experimental justification. It was obtained, however, as a direct consequence of the ether theory of light, which makes the velocity depend only upon the properties (*i. e.*, elasticity or electrical nature) of a stationary transmitting medium, and therefore, as with sound or other wave motions, independent of the velocity of the source. Until within a few years the ether theory had been so extraordinarily successful in explaining even the most complicated phenomena of optics, that we should have accepted any of its experimentally unproved conclusions without hesitation. At the present time, however, since the experiments of Michelson and Morley and of Trouton and Noble stand in such direct contradiction to the predictions of the ether theory, we have no hesitation in considering any other assumption as to the velocity of light, which, although not in accord with the ether theory, would free us from the complications introduced by the theory of relativity.

Such an alternative assumption, as to the velocity of light, which

[1] The first postulate of relativity practically denies the existence of any stationary ether through which the earth for instance might be moving. On the other hand, the principle that the velocity of light is unaffected by a motion of the source is closely bound up with the idea that light is transmitted by a stationary ether which does not partake in the motion of the source. It is not surprising that the combination of two principles based on seemingly contradictory ideas should give to the second postulate its extraordinary content.

would cause none of the complications introduced by the second postulate, *is* possible. The velocity of light and other electromagnetic propagations might not be independent of the motion of the source, but their velocity and that of the source might be additive. This assumption would be very simple, would be no contradiction to the first postulate of relativity, and would directly explain all our failures to detect an ether drift. It is not difficult, for example, to see that this assumption does directly explain the Michelson-Morley experiment. If O is a source of light and A and B are mirrors placed a meter away from O, the Michelson-Morley experiment shows that the time taken for light to travel to A and back is the same as for the light to travel to B and back, in spite of the fact that the whole apparatus is moving through space in the direction of $O-B$, due to the earth's motion around the sun.

Fig. 2.

The above assumption, however, would require exactly this result, since it says that light travels out from O with a constant velocity in all directions with respect to O, and not with respect to some ether through which O is supposed to be moving. It is in fact obvious, in general, that this principal if true would lead to the simplest kind of relativity. For, if light or any electromagnetic disturbance which is being emitted from a source, partakes in the motion of that source in such a way that the velocity of the source is added to the velocity of emission, then a system consisting of the source and its surrounding disturbances acts as a whole and suffers no change in configuration when the velocity of the source is changed.[1] The possibility of such an assumption has already been pointed out in various places.[2] Nevertheless, in spite of the ap-

[1] There would, of course, be a temporary change in configuration during acceleration.

[2] Lewis and Tolman, loc. cit. Comstock, PHYS. REV., 30, 267, 1910. Tolman, PHYS. REV., 30, 291, 1910. The assumption has also been adopted in a modified form

parent relief which would follow the adoption of this idea, the evidence which we are about to present is all in favor of the older idea of the velocity of light which has led to the second postulate and the complicated theory of relativity.

THE DOPPLER EFFECT.

The Doppler effect, that is the influence which a motion of the source of light has upon the *frequency* of the emitted ray, has been the subject of much refined experimental work. It is natural to consider what relation this effect bears to our own problem, concerning the influence which a motion of the source of light has upon the *velocity* of the emitted ray.

If we have a body which is emitting the periodic disturbance which we call light, and then start the body in motion towards an observer, it is evident that the frequency with which the disturbances will reach the observer will be increased, since, in order to get to him, each successive disturbance has to travel a less distance than the one preceding it. As we shall see, this change, in the frequency of the light, will be produced in nearly the same amount, whether or not the velocity with which the light travels towards the observer is affected by the motion of the source. We shall find, however, that, with regard to the actual distance in space between successive disturbances (*i. e.*, wave-length) the two hypotheses as to the velocity of light lead to quite different conclusions. This difference presents a method of deciding between the two rival hypotheses.

We shall proceed to an actual derivation of the effect which a motion of the emitting source has upon the wave-length of light, first, assuming that the velocity of the light is independent of the motion of the source, and then that the velocities of the light and source are additive.

It is evident that we must make no use of arguments based on preconceived notions as to the nature of light, but must restrict ourselves to purely kinematic considerations which would be equally

by Campbell who considers that light is a transverse vibration in the Faraday tubes attached to a vibrating electron, and since the tubes partake in the motion of the electron, the velocity of light is evidently dependent on the velocity of the source. (See Modern Electrical Theory, University Press, Cambridge, 1907.)

true of any periodic disturbance. Let c be the velocity with which the disturbance travels, in this case that of light, λ_0 and n_0 the observed wave-length and frequency (of some particular line in the spectrum), when the source is at rest with respect to the observer, and λ and n the same quantities after the source has been set in motion towards the observer with the velocity v. Let us first consider—

Case I.—The velocity of light is independent of the velocity of the source. Then,

$$\lambda = \lambda_0 - \frac{v}{c}\lambda_0 = \lambda_0\left(1 - \frac{v}{c}\right).$$

This is the ordinary formula for the Doppler effect and the derivation is simple, since it is evident that while an emitted wave-front is moving forward the distance λ_0, the source itself has moved forward the distance $\lambda_0 \cdot v/c$, and the next wave-front to leave the source will have gained this distance over the earlier one. The frequency of the light will be equal to the velocity divided by the wave-length.

$$n = \frac{c}{\lambda} = \frac{c}{\lambda_0}\left(\frac{c}{c - v}\right) = n_0\frac{c}{c - v}$$

$$= n_0\left(1 + \frac{v}{c} + \frac{v^2}{c^2} + \cdots\right).$$

Case II.—The velocity of light and that of the source are additive, the velocity with which light passes the observer is $v + c$.

$$\lambda = \lambda_0.$$

This result is evident on inspection, since, under the conditions assumed, the velocity of light relative to the source is always the same, and the source and its surrounding disturbances move together as a whole, suffering no permanent change in configuration when the velocity of the source is changed. In detail, however, we see that, with respect to the observer, an emitted wave front moves forward the distance $\lambda_0 \cdot (c + v)/c$ during the interval of time which elapses before the next wave front leaves the source, and during that time the source has moved forward the distance

$$\frac{v}{c + v}\lambda_0\frac{c + v}{c} = \lambda_0\frac{v}{c},$$

making

$$\lambda = \lambda_0 \frac{c+v}{c} - \lambda_0 \frac{v}{c} = \lambda_0 .$$

The frequency of the light is evidently

$$\frac{c+v}{\lambda} = \frac{c+v}{\lambda_0} = \frac{c}{\lambda_0}\left(\frac{c+v}{c}\right) = n_0 \left(1 + \frac{v}{c}\right),$$

a result which except for second order terms is identical with that obtained for Case I.

To sum the matter up, when the source is set in motion, whichever of the two hypotheses as to the velocity of light is true, the frequency of the light will be changed by practically the same amount, but the wave-length will be changed if one of the hypotheses is true and entirely unaffected if the other is true. Light emitted from moving sources, whether they are astronomical bodies or the moving mirrors arranged by Belopolsky, unquestionably does show the Doppler effect. We must investigate whether both the frequency and the wave-length are changed by the motion of the source.

The determination of the Doppler effect is made by a measurement of the displacement of some particular line from its normal position in the spectrum. When the spectrum is produced by a prism it is difficult to say whether the position of a given line would depend upon its frequency or its wave-length. Spectroscopic measurements made with the help of a grating are, however, actual determinations of wave-length. Since such measurements do show a change in wave-length of the light from many of the stars, and especially in light coming from the approaching and receding limbs of the sun, where the velocity of rotation is known from observations on the sun spots, we have, at first sight, strong evidence in favor of our first hypothesis that the velocity of light is independent of the source.

We must notice in these experiments, however, that the measurements of wave-length are made only after the light has been reflected from the surface of the grating and consider the possibility that this reflecting surface would act as a new source, giving to

the reflected beam the ordinary velocity of light as from any stationary source. If the surface of the grating should act in this way, then the reflected light would have the same velocity whichever of our hypotheses were true. Moreover, except for second-order terms, both hypotheses have led to the same conclusions as to the frequency of the light,[1] and since the wave-length of light is completely determined by its frequency and velocity, such an action of a reflecting surface would prevent our distinguishing between the two hypotheses. We can, however, draw one useful conclusion from our consideration, namely, that if the velocity of light does depend on the velocity of the source, then a reflecting mirror acts as a new source, and the velocity of the reflected beam depends only on the motion of the mirror.

Before proceeding to the consideration of an experiment in which we shall make use of the principle just derived, let us consider whether a transmission grating would also act as a new source of light and destroy any original difference between the velocities of light, for example, from the two limbs of the sun. In the next section we shall consider more in detail certain experiments of Fizeau and of Michelson concerning the velocity of light in media which are moving with respect to the source of light. We are, however, led by them to the conclusion that an original difference in the velocity of light from the two limbs of the sun would be only about one half obliterated by passage through a plate of glass, and hence the use of transmission gratings for deciding between the two postulates as to the velocity of light would be possible. At the present time very excellent transmission gratings are obtainable, being replicas of original Rowland gratings. It is desirable that observations be made with apparatus in which the light suffers no reflection in order to definitely settle the matter.

Experiments on the Velocity of Light from the two Limbs of the Sun.

The fact that a mirror acts as a new source of light led me to the construction of a very simple apparatus for comparing the velocity of light from the approaching and receding limbs of the

[1] The frequency, would, of course, not be changed by reflection.

sun. A tube mounted on a small telescope stand was provided with a slit, an observing eye piece, and a mirror (plate glass) as shown in the diagram. The apparatus was adjusted to produce interference fringes between the light from the slit and from its mirror image, a method employed by Dr. Lloyd. The mirror was about 25 cm. in length and the distance from the end of the mirror to the eye piece was about 10 cm.

Let us suppose an interference fringe at the point *f* produced by the combination of a ray of light *sf*, coming direct from the slit and another *smf* which has suffered reflection from the mirror. If now

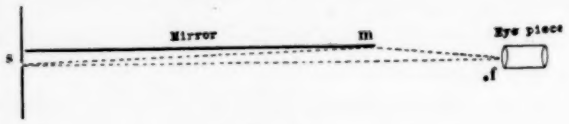

Fig. 3.

the velocity of light *were* dependent on the velocity of its source, and we set the source in motion towards the slit, the time taken for a given wave-front to travel from the slit to the eye piece along the path *sf* would be increased by a greater amount than along the path *smf*, since for the distance *mf*, which in the apparatus was at least 10 cm., the ray is traveling with the normal velocity of light as from a stationary source, while for the whole path *sf* the light is traveling with an increased velocity. We should thus expect a shift in the fringes to accompany a change in the velocity of the source.

A lens of about 9 cm. aperture and 80 cm. focus was used to throw an image of the sun (about 8 mm. in diameter) on the apparatus, and light first from one and then from the other limb of the sun allowed to enter the slit. No shift in the fringes was observed. We may easily calculate the magnitude of the expected effect. Suppose we are receiving light from the approaching limb of the sun, the velocity of the ray *sf* would be greater than that of the reflected ray by about 1.5 km. per second,[1] so that a given wave-front in traveling the ten centimeters from *m* to *f* would fall behind the corresponding one which travels along *sf* by

[1] The peripheral velocity of the sun due to its rotation is a little under 2 km. per second.

$$\frac{1.5}{300{,}000} \times 10 = .5 \times 10^{-4} \text{ cm.}$$

We obtain double the effect, or 10^{-4} cm., in changing from the approaching to the receding limb of the sun, and considering 5×10^{-5} as an average wave-length, we should expect a shift of about two fringes. Since no shift was observed, we have strong evidence that the velocity of light is independent of the velocity of its source.

To complete the discussion, it is necessary to consider the effect of the lens (and the earth's atmosphere) through which the light has to pass before reaching the slit. At first sight, it might seem as if an original difference between the velocities of light from the two sources would be obliterated by passage through a stationary medium. The experiments of Fizeau and of Michelson, however, give us data for calculating the effect upon the velocity of light of relative motion between the source of light and a transmitting medium.

If c is the velocity of light in vacuo, μ the index of refraction of the medium and v the velocity with which the medium is moving towards the source, we have the velocity of light in the medium equal to $c/\mu \mp v\theta$, where θ is some fraction which must be determined experimentally. The considerations of Fresnel and others have led to the expression for any medium $\theta = (\mu^2 - 1)/\mu^2$, an equation which was very closely verified for water in the experiments referred to. The velocity of light in the lens may now be calculated.

$c/\mu \mp v\theta$ is the velocity of light with respect to the source; with respect to the medium, it will be $c/\mu \pm v(1 - \theta)$. For glass, putting $\mu = 1.5$, we have $\theta = (\mu^2 - 1)/\mu^2 = 0.51$. The velocity of light from the approaching limb will be $c/\mu + 0.49v$ and from the receding limb $c/\mu - 0.49v$, where v may be taken as 1.5 km. per second. We now see that, even if the light after leaving the lens did not regain its original velocity, the difference in the velocities of the light from the two limbs of the sun would still be about 1.5 km. per second, which would give a shift of one fringe in the experiment performed.

As a result of the experiment, we conclude that the velocity of

light from the two limbs of the sun is the same. The possibility that an original difference in velocity would be destroyed when the light reached the neighborhood of the earth is not entirely excluded. Nevertheless, the experiments of Fizeau and Michelson which we have just discussed seem to show that the presence of air or other transmitting medium *would not* completely destroy such a difference. Furthermore, the experiment of Sir Oliver Lodge has led us to expect no change in the velocity of light produced by the neighborhood of large masses such as the earth. It may also be pointed out in this connection, that, up to the present time, no astronomical data of any kind have been found which are in disagreement with the principle that the velocity of light is independent of that of the source. For example, it has been shown by Comstock[1] that a difference in the velocity of light from approaching and receding stars would lead us to expect irregularities in the observed orbital motion of double stars which have never been detected. Certainly, until further evidence is presented, we may best accept that principle regarding the velocity of light which has led to the second postulate of relativity.

We shall now consider an entirely different method of proving the second postulate of relativity.

The Kaufmann-Bucherer Experiment.

Certain conclusions of the theory of relativity have been quantitatively verified by the experiments of Kaufmann and of Bucherer on the mass of the β particle. It has already been stated in the article of Lewis and Tolman referred to above that this experimental fact may itself be used for the reverse process of deducing the second postulate, and a method of proof was worked out at the time that paper was published. It is very desirable to consider this proof since it includes *a deduction, without the help of the second postulate, of all the changes in the units of length and time,* to which the theory of relativity has led, and finally gives a proof of the second postulate itself.

Let us suppose an electron ε at rest and an electron ε' moving past it with the velocity v.

[1] Comstock, loc. cit.

Let ε' be originally so far away that the electrons do not appreciably repel each other, and let the experiment continue until they are again far apart. Owing to their mutual repulsion, while they are within each other's sphere of influence, electron ε receives a certain component velocity μ in a direction perpendicular to the general line of motion of the two systems, and ε' also receives a transverse velocity μ'. These transverse velocities are made small compared with the relative velocity of the two systems. If the mass of an electron at rest is m, then by the Bucherer experiment, the electron ε', which we consider in motion, will have the mass $m/\sqrt{1-\beta^2}$, where β is v/c, so that the electrons have respectively received the transverse momenta mu and $mu' \cdot /\sqrt{1-\beta^2}$.

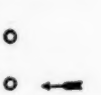

Fig. 4.

By the principle of the conservation of momenta

$$mu = m\frac{1}{\sqrt{1-\beta^2}}u'$$

and

$$u' = \sqrt{1-\beta^2}u.$$

In other words, the electron ε', since it has a larger mass than electron ε does not receive so great a transverse velocity. If, however, an observer had been traveling along with ε', he would have been entirely unable to detect this fact that his electron had received a smaller velocity than the other one, since the first postulate of relativity states that no measurements are possible by which an observer may detect that he is in absolute motion. Since, now, to this moving observer, the velocity seems larger than it does to an observer at rest in the ratio $1 : \sqrt{1-\beta^2}$, the second which the moving observer uses must be longer than a "stationary" second in the same ratio $1 : \sqrt{1-\beta^2}$.[1]

Having obtained the ratio between the units of time in a moving and stationary system, let us deduce the Lorentz shortening. Suppose two systems a and b moving past each other with the velocity v and two observers A and B on the systems. A makes two marks

[1] It is evident that the difference of opinion of the two observers as to the transverse velocity of the electron could not be reconciled by assuming a difference in the transverse units of length, since it is perfectly possible for the observers to make a direct comparison of meter sticks held perpendicular to the line of motion of the systems. (See Lewis and Tolman, loc. cit.)

on his system a centimeter apart, in the line of motion of the system, and requests B to determine the time it takes for a point on b to pass from one mark to the other. B also makes two marks on his system a centimeter apart, and A finds that it takes the same number of seconds to pass from one mark to the other as B found in his own similar experiment. Any other outcome of the trial would be contradictory to the first postulate of relativity. If, however, we again arbitrarily consider A to be at rest, A's seconds are shorter than B's in the ratio $\sqrt{1-\beta^2} : 1$, and hence the two points on B's system must have been nearer together than those on A's in this same ratio $\sqrt{1-\beta^2} : 1$. In other words, the "moving" centimeter in the longitudinal direction is shorter than a "stationary" one in the ratio $\sqrt{1-\beta^2} : 1$.

We have now derived the change in the units of length and time in a moving system, with the help of the Bucherer experiment. Before we can derive the desired principle regarding the velocity of light, we must go one step further and find out how the clocks are set in a moving system.

The observer B who is in motion with the velocity v past a system a which we consider at rest, lays off a length of one centimeter on his system in a longitudinal direction, and with the help of two clocks, one at each end of the centimeter, notes the time taken for a point on a to pass from one end of this centimeter to the other. He obtains, of course, the time $1/v$. Since, however, his seconds are longer and his centimeters shorter than stationary ones in the ratio $1 : \sqrt{1-\beta^2}$ we would have expected him to obtain the time $(1-\beta^2) \cdot 1/v$, and we can account for his obtaining the longer time $1/v$, only by the assumption, that, in a moving system, a clock 1 cm. to the rear of another is set ahead by the amount

$$\frac{1}{v} - \frac{1}{v}(1-\beta^2) = \frac{v}{c^2} \text{ seconds.}$$

We are now ready to deduce our principle as to the velocity of light. Consider a source of light and an observer B who measures the velocity of the light coming from this source. If the observer B is at rest and marks off a length of one centimeter in the path of light, he finds, of course, that the light takes the time $1/c$ to pass from one mark to the other. We wish to prove, however, that

the relation which we have just derived, requires that the observer will also obtain the time $1/c$ even if he is in motion towards or away from the source.

Let us suppose that the observer B is moving towards the light with the velocity v, the velocity with which light is passing his system is $v + c$, the time taken for it to travel over a centimeter length is $1/(v + c)$. Since, however, the centimeters which B marks off are shorter than "stationary" ones in the ratio $\sqrt{1 - \beta^2} : 1$, and the seconds which he uses are longer in the inverse ratio, the time required in his units would be $(1 - \beta^2) \cdot 1/(v + c)$, and further since the rearmost clock is set ahead by the amount v/c^2, we finally conclude that B will obtain the time $(1 - \beta^2) \cdot 1/(v + c) + v/c^2$ which reduces to $1/c$. We thus conclude that the velocity of light appears the same whatever the motion of the observer, or by the first postulate of relativity whatever the relative motion of the source of light and the observer, and have obtained a proof of the second postulate of relativity with the help of the Bucherer experiment.

In this connection, it must be again pointed out that the Kaufmann-Bucherer experiment may not really indicate an increase in the mass of an electron in motion. It is, at first sight, equally possible that the forces acting on an electron in rapid motion through electrostatic or magnetic fields are not as large as those calculated on the basis of Maxwell's fifth equation, since its application to high velocities certainly lacks experimental justification. The balance of all the evidence which has been presented, however, is in favor of the second postulate of relativity.

Summary.

In this paper it is shown that the extraordinary conclusions of the theory of relativity are forced on it by the second postulate of relativity. This postulate is obtained by combining the first postulate of relativity with the principle that the velocity of light is independent of the velocity of the source. The alternative hypothesis that the velocity of light and the velocity of its source are additive would lead to none of the complications of the theory of relativity. Two methods are presented for deciding between the two hypotheses.

The first method would be to measure the wave-length of light from some moving source, since it is shown that the first hypothesis would lead us to expect a change in the wave-length of light from moving sources and the alternative hypothesis would not. The existing measurements of the wave-length of light from moving astronomical sources, made with reflection gratings, are not of such a nature that we can definitely decide that the velocity of light is independent of the motion of the source. We can, however, state that, if the alternative hypothesis is true, then a reflecting surface acts as a new source of light and light coming from such a surface has the normal velocity as from any other stationary source. It is further shown that measurements with a transmission grating would more nearly allow a definite decision of the question.

The principle that the velocity of light from a stationary mirror is the same as for light from any stationary source leads to a second method of deciding between the two hypotheses as to the velocity of light. An apparatus is described in which interference fringes were produced between light which had come direct from the sun and light which had first suffered reflection. Since the velocity of the reflected ray would in any case be unaffected by the motion of the original source, we should expect a shift in the position of the fringes to accompany a change in the velocity of the direct ray. No shift in the fringes was observed in examining light first from the approaching and then from the receding limb of the sun. We conclude that the velocity of light from the two limbs of the sun is the same, which confirms the principle that has led to the second postulate of relativity.

An entirely different method of proving the second postulate of relativity is afforded by the results of the Kaufmann-Bucherer experiment. In fact, by a combination of the first postulate of relativity with the principle that the mass of a moving electron is greater than that of a stationary one in the ratio $1 : \sqrt{1 - \beta^2}$, it was found possible to deduce *all the conclusions of the theory of relativity as to the units of length and time in a moving system*, and finally to deduce the second postulate of relativity itself.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
BOSTON, MASS.,
January 6, 1910

AN EXPERIMENTAL DETERMINATION OF THE CHARGE OF AN ELECTRON BY THE CLOUD METHOD.

By L. Begeman.

DETERMINATIONS of e have been made by J. J. Thomson and H. A. Wilson at the Cavendish Laboratory, by methods that were quite different, although both obtained their data from observations of ionized clouds produced by the rapid expansion of supersaturated air in a fog chamber.

Thomson's method is in brief as follows: To secure a constant source of radiation radium was used as the ionizing agent. This radium was suspended at a given distance above the fog chamber and maintained continuously in the same position during the observations. C. T. R. Wilson's device for rapid expansions[1] was used to produce the clouds in the fog chamber. The clouds were produced between two plane electrodes maintained at a constant difference of potential. If n is the number of droplets per c.c. in the cloud and e the charge on each, then the charge of the cloud per c.c. is ne. Furthermore, if u is the mean velocity of the positive and negative ions in the given electric field, then the current through unit area of the ionized gas is neu. Hence it is only necessary to know the current and u to determine the value of ne. Thomson used Zeleny's and Rutherford's determinations of u. The method of C. T. R. Wilson was used to determine the number of particles of water vapor in a unit volume of the cloud. Assuming this to be the same as n Thomson obtained at once a value for e. The mean of his determinations gave $e = 3.6 \times 10^{-10}$ E.S. units.

In the discussion of his work Thomson states that when the expansion is greater than 1.31 (this corresponds to a dp., *i. e.*, a fall in pressure, of 19 cm. of Hg.), positive as well as negative ions are caught, and that the number is about twice as many as is obtained

[1] Conduction of Electricity through Gases, 1906. Also Phil. Mag., Vol. 5, p. 346.

at expansions varying from 1.27 (dp. = 16 cm.) to 1.29 (dp. = 17 cm.). He says also that when the expansion is greater than 1.33 (dp. = 19 cm.), the number of nuclei caught by the cloud does not depend upon the amount of the expansion. Thomson's measurements were all made at expansions of about 1.33, at which point he assumed that every droplet in the cloud carried but one ion either positive or negative.

According to Carl Barus[1] the nucleation induced by radium increases rapidly with increasing expansions from 1.33 to 1.4 (dp. = 22 cm.). Above 1.4 the nucleation gradually increases and a maxi-

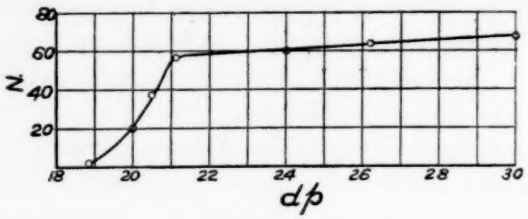

Fig. 1.

mum is not attained until the dp. is 34 cm. Fig. 1 is a graph by Barus showing the nucleation due to radium at various expansions. In this graph the ordinates represent the nucleation (the number of condensed particles per cubic centimeter in the cloud in units of 1,000); while the abscissæ represent the dp. in centimeters. As will be noted, the nucleation begins at a dp. of 19 cm. and gradually approaches a maximum after a dp. of 22 cm. is reached. Barus observed that at the lower expansions nuclei carried a variable number of ions. My own determinations at an expansion of 1.33 also indicate this fact. In our later work Prof. Millikan and myself found multiple charged drops at all pressures.

Fig. 2 shows a curve very similar to Barus's, but one which was obtained in an entirely different manner. In this figure the ordinates give the times it took for the cloud formed in the fog chamber by expansion to fall a distance of 2 millimeters under the action of gravity. It will be seen that as dp. increased from 16 cm. to 24 cm. the time increased from 3 seconds to 5.2 seconds. At the low expansion the droplets were heavy, falling rapidly. As the dp. in-

[1] May, 1908, Publication of the Carnegie Institution.

creased the droplets became smaller with a correspondingly diminished velocity. The density of the clouds also increased rapidly with increasing pressure. Comparing the curve of Fig. 2 with that of Barus in Fig. 1, we note that the velocity under gravity of an ionized cloud varies, approximately, inversely as the nucleation.

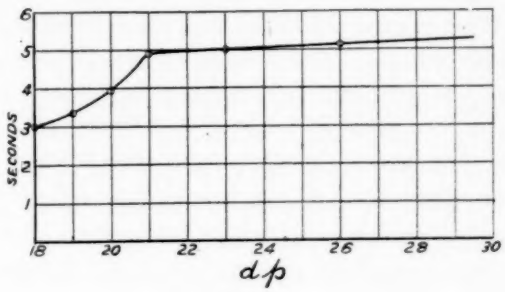

Fig. 2.

H. A. Wilson's method[1] of determining e has the advantage over J. J. Thomson's in that it is not necessary to know the number of particles in the ionized cloud. All that one needs to know is, first, the velocity of the cloud under the action of gravity, and, second, the velocity under the combined action of gravity and a static field of known strength. Wilson obtained for his working formula:

$$e = 3.1 \times 10^{-9} \cdot \frac{g}{X}(V_1 - V)V^{\frac{1}{2}}.$$

Wilson's determinations varied from 2×10^{-10} to 4×10^{-10} and his average was 3.1×10^{-10} E.S. All of his determinations were taken at a dp. of 17 cm.

There are some serious objections to Wilson's experiment. He used X-rays as his source of ionization, and we know that such a source may be extremely variable. It is probable that this variability of the source accounts for the marked differences in the velocities of his clouds for successive observations. In a series of eleven sets of observations we find that the timing of his clouds varies from 12 seconds for a distance of 5 millimeters to 33 seconds for a distance of 5 millimeters. In my own work with radium as a

[1] Philosophical Magazine, Series 6, Vol. 5, 1903.

source of ionization, the timing of the clouds was practically constant for any given difference of pressure.

Again, as has been stated above, Wilson worked at a dp. of 17 cm., which, according to Barus, is right within the region of persistent nuclei for X-rays. Barus observes that these persistent nuclei induced by X-rays are ionized, carrying a variable number of ions; one, two, or three, perhaps. He notes also that their number in the fog chamber varies with the time of exposures and also with distance of the X-ray bulb from the fog chamber. Their masses accordingly vary considerably, which fact no doubt accounts for the different layers witnessed by Wilson in his clouds. A serious objection to Wilson's work is seen in the fact that the number of persistent nuclei vary with the time of exposure. If the time of exposure was not the same in all determinations his velocities would necessarily vary considerably as his data show that they did. X-rays were first used as the ionizing source for the determination of e at the Ryerson Laboratory, but the results were so unsatisfactory that radium was substituted.

The data for the determination of e published in 1907 by the writer in conjunction with Prof. R. A. Millikan were obtained under conditions which preclude in a measure the criticisms on former determinations. In this work Wilson's method was followed and 200 mg. of a one per cent. compound of radium was used as the ionizing agent. Our published mean value was 4.06×10^{-10} E.S. units. Since then the writer has gone carefully over the data and has obtained a corrected mean of 4.09×10^{-10}. In these calculations the constant 3.1 was used as the multiplying factor in the working formula. According to the recent work of Prof. R. A. Millikan[1] the constant should be 3.422. Correcting the above determination for the error in the constant and also for one and one half per cent. error in the voltmeter used we obtain a mean value for e in this early work equal to 4.59×10^{-10} E.S. units.

Beginning with the summer quarter 1908, the work of making observations for the determinations of e was continued. The problem was attacked in a manner quite different from that of the fall of 1907. The observations in the fall of 1907 were taken by timing

[1] Phil. Mag., February, 1910.

the upper surface of the falling clouds through a distance of two millimeters. In 1908 the timing was taken on individual droplets or clusters that were caught by the eye in the lower surface of the cloud. This was somewhat difficult to do at first and required repeated practice to develop the perceptive skill necessary for reliable data. In all some 68 separate determinations of e were made under varying conditions. These determinations comprised about 400 pairs of observations with field off and on. The values for e in these determinations varied from 3.47×10^{-10} to 5.85×10^{-10} and the mean of all of them was 4.54×10^{-10} E.S. units.

One of the interesting features of this summer's work was the taking of a set of data on positively charged droplets. The terminals of the field were reversed so that the top electrode in the fog chamber was positively charged when the field was on. These data show conclusively that the unit of positive electrification is the same in magnitude as the negative. Below are given the complete data of some dozen determinations of the unit positive charge. The experimental conditions in securing these data were exceptionally good. It will be noted, however, that the rates of fall were quite slow, so that one should expect an error due to evaporation.

In all the data below the distance between the electrodes of the fog chamber was .575 cm. The distance between the cross hairs in the micrometer telescope was .3 cm. The fall of pressure upon expansion in the fog chamber was about 17 cm. In each set of data the times are given for the field off and on, also the voltage and the calculated value of e.

About the first of June, 1909, the apparatus for the determination of e by the "cloud method" was again set up in the Ryerson Laboratory by the writer. A new adjustable spring for the control of the exhaust valve was devised and other improvements were made so that the mechanical action of the apparatus was excellent in every respect.

The apparatus as finally set up is shown in Fig. 3. A is the fog chamber which contains the two electrodes between the plates of which the clouds were produced. The lower plate of the electrodes is slightly smaller than the upper. In the original apparatus these plates were of the same size, but it was found that by making

Off.	On.		Off.	On.	
11.0 secs.	8.0 secs.		11.4 secs.	8.4 secs.	
11.0	7.6		11.4	8.4	
11.0	8.0	2,265 volts.	11.4	8.0	2,265 volts.
11.0	7.8	$e=4.55\times10^{-10}$	11.8	8.4	$e=4.11\times10^{-10}$
11.0	8.0				
11.0 secs.	8.0 secs.		11.2 secs.	8.0 secs.	
10.8	7.6		11.0	8.0	
11.0	8.0	2,265 volts.	11.4	8.2	2,265 volts.
11.2	7.8	$e=4.55\times10^{-10}$	11.4	8.2	$e=4.32\times10^{-10}$
11.0	8.0				
12.0 secs.	8.4 secs.		12.0 secs.	8.4 secs.	
12.0	8.4		12.0	8.4	
11.8	8.4	2,215 volts.	12.0	8.4	2,215 volts.
12.0	8.4	$e=4.20\times10^{-10}$			$e=4.41\times10^{-10}$
13.4 secs.	9.0 secs.		12.0 secs.	8.4 secs.	
13.2	9.2		11.6	8.4	
13.0	9.0	2,165 volts.	11.0	7.8	2,340 volts.
13.2	9.0	$e=4.28\times10^{-10}$	10.8	7.8	$e=4.18\times10^{-10}$
13.4	9.2				
12.0 secs.	8.6 secs.		12.6 secs.	8.6 secs.	
11.8	7.4		12.4	8.2	
12.0	8.0	2,265 volts.	12.2	8.0	2,240 volts.
11.6	7.8	$e=4.98\times10^{-10}$	12.0	8.2	$e=4.78\times10^{-10}$
12.0	8.2				
13.2 secs.	9.0 secs.		14.0 secs.	9.0 secs.	
13.0	8.8		14.0	9.2	
13.2	8.6	2,215 volts.	14.0	9.4	2,215 volts.
13.2	9.0	$e=4.60\times10^{-10}$	14.0	9.2	$e=4.27\times10^{-10}$
13.2	8.8		14.0	9.2	

one of them slightly smaller, a swirling effect on the cloud that continually occurred was avoided. The diameter of the electrode plates was about two centimeters. The fog chamber was filled with water up to within about two centimeters of the lower electrode plate. A jacket T_1 was fitted around the fog chamber containing water. Another jacket T_2 will be seen around the expansion chamber C. The purpose of these jackets was to maintain as uniform a temperature as possible in the air space of the fog chamber and the expansion chamber C, and also to permit the change of temperature when desired.

The expansion chamber C contains a piston valve like that used

by Wilson in his work. This valve, at first, caused considerable trouble owing to the fact that it did not come down squarely upon the rubber stopper at the bottom. It was necessary to grind with care the lower edge of the piston valve and to have it made so as to fit snugly in the expansion chamber.

M is a mercury manometer gauge which can be connected by means of the stop-cock *C* with the vacuum chamber, or cut off as one pleases. In practice it was found advisable to close *C* when an expansion was made and then open it directly after taking an

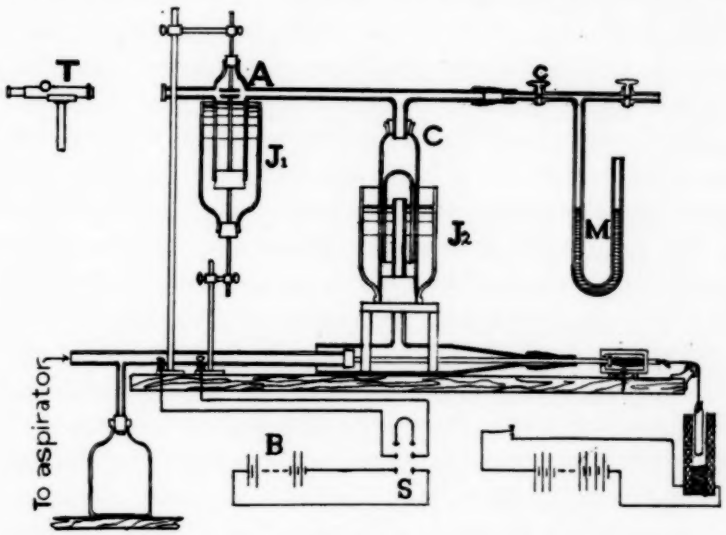

Fig. 3.

observation on the cloud to determine the difference in the barometer pressure.

The apparatus for procuring a vacuum consisted of an aspirator as indicated on the left of the diagram. The aspirator connected with a large bottle as shown. From the bottle a tube led under the expansion chamber *C*. The end of the tube was covered with a rubber stopper held firmly in place by a coil spring as shown on the right. The coiled spring was operated by an electric solenoid connected up in a storage battery circuit. In the final work an iron core was placed in the bottom of the solenoid to increase the suddenness of the expansion. To produce an expansion the battery circuit

is closed. This produces a sudden pull on the coiled spring, drawing the rubber stopper off the end of the tube leading to the aspirator bottle. The result of such an operation is to produce a vacuum under the piston, which descending produces the desired expansion in the fog chamber.

The terminals of the electrodes in the fog chamber were connected with a battery of 1,600 small storage cells as shown at *B*. The switch *S* used to throw the field off and on was generally operated automatically by means of two telegraph relays. Each relay took care of one wire leading to the terminal. The action of the relays was so timed that the field was thrown on immediately after the cloud was formed in the fog chamber. The battery terminals could also be operated by hand.

The observations on the clouds were taken through a micrometer telescope as shown in the upper left side of the figure at *T*. The telescope was mounted on a cathetometer support. The fog chamber had a projecting tube on the side towards the telescope which was about four centimeters in length and one centimeter in diameter. The end of the tube was covered with a smooth piece of glass held in place with dentist's cement. The clouds were viewed through the projecting tube. When the condensed moisture obscured the vision, an incandescent lamp held for a moment against the plate glass of the projecting tube dispelled the fog without affecting the temperature in the main body of the fog chamber. In observing, the upper surface of the cloud was timed as it passed successively two parallel cross-hairs set at a given distance apart.

The principal objections to the "cloud method" are as follows: First, it is impossible to say that the successive observations are taken on exactly similar clouds and under exactly similar conditions. To this I can only say that the experimental data certainly show that the successive clouds for any given date must have been very similar—at least within the limits of more palpable experimental errors. A casual scrutiny of these data will show that the timings of the successive clouds for any given set of observations are practically uniform, showing no more variations than one would expect in the use of a stop-watch in timing any constantly recurring phenomena.

The second more serious objection is the probable error due to evaporation. One might expect the evaporation to be considerable on the ground that there must be a considerable drop of temperature in the fog chamber on exhaustion. Prof. R. A. Millikan, after the taking of my data, thoroughly investigated the temperature phenomena under the conditions of the experiment. The results of his investigation are clearly presented in his discussion of the "balanced drop" method.[1] He shows that the temperature drops only .8° C. upon expansion and recovers in five or six seconds to within 0.5° C. of the room temperature. In my experimental work the cross-hairs in the observing telescope were so set that at least 6 or 7 seconds elapsed before the beginning of an observation. Consequently there should not have been in my experiments any rapid evaporation because of a rapid rise in temperature during the time of observation.

To verify this a large number of observations were made to see if there was any negative acceleration in the motion of the descending clouds. Evaporation, by diminishing the size of the drops would, according to Stokes' formula, produce a retardation in the motion of the droplets. The data immediately following were taken to determine any probable error due to evaporation. The first column shows the time it took the clouds to fall through a distance of one millimeter between the cross-hairs of the telescope. For the second column the distance is two millimeters and for the third, three millimeters.

	Field Off.	Field On.	Field Off.	Field On.	Field Off.	Field On.
	3.6	2.8	7.0	5.0	10.6	7.6
	3.6	2.8	6.8	5.2	10.6	7.4
	3.6	2.4	6.8	5.0	10.8	7.8
	3.4	2.6	7.4	5.2	10.6	7.4
	3.4	2.4	6.8	5.0	10.6	7.6
	3.6	2.6	7.0	5.0	11.0	7.6
	3.4	2.6	7.2	5.2	11.0	7.8
Mean	3.51	2.57	7.00	5.09	10.74	7.6

The means for the field off when clouds are falling under gravity

[1] Phil. Mag., February, 1910.

are 3.51, 7.00 and 10.74 which are almost exactly in the ratio of 1 : 2 : 3. The means for the field on are 2.57, 5.09 and 7.6 which show the same relation. Many observations of this kind were taken and no evidence of retardation was noticed on the cloud masses — at least none that could not have been due to the natural errors of timing.

It is not intended to convey the idea that there is no evaporation in the clouds, for experience has shown otherwise. Probably not at any one time did we have clouds comprised of drops all of which were of uniform size. The work by the "balanced drop" method showed conclusively that when drops of different sizes were present, the small, weakly charged ones evaporated rapidly in the presence of the larger and heavily charged drops. This is to be expected also from theoretical considerations. One would conclude then that the observations on the cloud layers made up of singly charged drops would be quite unreliable because of evaporation. This is perhaps to be acknowledged. Yet there are reasons why it ought not to be as serious as it at first appears. In the first place when observing by Wilson's method the drops are accelerated by the field and not retarded as in the "balanced drop" method. The result would be that the multiply charged drops would be pulled quickly out of the field leaving the slowly moving part of the cloud made up of small droplets carrying unit charges. In fact, there was always a time interval before the beginning of an observation on a cloud surface passing the first cross-hair. During that time interval the large multiples were drawn out by the field.

The above argument, of course, will not hold when the field is off and the cloud falls under the action of gravity alone. However, it is to be said that there is a difference between the mass velocity of a cloud and the velocity of a single drop. A cloud affected by evaporation shows a cutting away of its upper surface due to the complete annihilation of the drops through evaporation at its surface layer. Perhaps this cutting away just neutralizes for short periods of observation the retardation due to the diminished size of the individual drops. For, it is evident that the cutting away of the upper surface of the cloud is equivalent to a virtual, positive acceleration.

We append another series of data for evaporation taken while working with Prof. R. A. Millikan. Each pair of observations was taken on the same cloud as it passed successively the three parallel cross-hairs of the telescope. These data fully corroborate the previous work. It would be difficult to infer from these figures that there is any evaporation. The first column in each case gives the time for the passage of the cloud through the first space and the second column the time for twice the distance or the entire space between the extreme cross-hairs. The cross-hairs were set for two equal spaces of 1.5 millimeters each.

Millikan.		Begeman.	
4.6 secs.	9.2 secs.	5.0 secs.	10.0 secs.
4.8	9.4	4.8	9.6
4.6	9.4	4.4	9.0
4.8	9.4	4.8	10.0
4.8	10.0	4.4	8.8
		5.2	10.0
		5.6	11.0

The following data were taken by timing the upper surface of the cloud first with the field off, then with the field on. In all the work we were careful to combine only such data for calculation which showed that the phenomenon was recurring under similar conditions. Frequently the phenomena would change suddenly, due to some defect in the mechanical manipulation of the apparatus. All of the data below were taken at the uniform difference of pressure of 17.5 centimeters, as this was thought to be the best working expansion. The temperature of the room was uniformly close to 26° C. In the data below V_0 stands for the velocity of the cloud under the action of gravity; V_1 for the velocity under the action of the field and gravity combined; R stands for distance between the cross-hairs for which the cloud was timed, and P stands for the distance between the electrodes. The constant 3.422 was used in these calculations.

During the course of the work it was noticed that the ionized clouds at an expansion of 17 cm. generally had two well-defined layers of different velocities when the field was on. The more rapid layer was, as a rule, better defined than the slower one. Sometimes as

Field.	V_0	V_1	R	P	e
1,605 volts.	.0292 cm.	.0387 cm.	.155 cm.	.482 cm.	4.92×10^{-10} E.S.
1,605	.0270	.0363	.155	.482	4.62
1,605	.0258	.0352	.155	.482	4.57
1,605	.0239	.0325	.100	.550	4.59
1,605	.0233	.0320	.100	.550	4.58
1,605	.0214	.0315	.200	.550	5.07
1,605	.0290	.0378	.150	.482	4.54
1,605	.0258	.0345	.200	.550	4.82
1,605	.0252	.0343	.100	.550	4.98
1,605	.0256	.0345	.200	.550	4.91
1,605	.0265	.0341	.100	.550	4.27
1,630	.0260	.0342	.200	.550	4.49
1,630	.0244	.0330	.100	.550	4.68
2,465	.0398	.0500	.200	.550	4.57
2,465	.0379	.0495	.300	.550	5.00
2,465	.0385	.0491	.200	.550	4.70
2,465	.0385	.0500	.100	.550	5.06
2,465	.0395	.0500	.200	.550	4.70
2,415	.0260	.0385	.200	.550	4.61
2,415	.0279	.0391	.200	.550	4.29
2,415	.0282	.0400	.200	.550	4.54
2,465	.0288	.0408	.200	.550	4.57
2,290	.0283	.0385	.100	.550	4.15
2,290	.0286	.0393	.200	.550	4.37
2,290	.0279	.0395	.300	.550	4.73
2,265	.0397	.0493	.200	.550	4.67
2,265	.0333	.0446	.200	.550	5.05

Mean $e=4.669\times10^{-10}$ E.S.

many as three distinct layers were seen when the field was on. The following is a series of data on the layers comprised of droplets whose charges consist of two units of electricity. The various values of e, as will be seen, are quite uniform.

The first six determinations were taken on clouds produced by a slow expansion. Under such conditions practically all of the drops in the cloud carry double charges. The rest of the determinations were taken on clouds showing well-defined double layers when the field was on.

These data should be more reliable than those upon the singles for two reasons. In the first place, the drops carrying multiple charges are the larger ones in the cloud and naturally tend to accumulate moisture at the expense of the smaller ones. For this

reason it is very probable that any evaporation which might take place in the cloud layer carrying doubles or multiples is completely offset by the moisture acquired as it falls through the mass of singles. At no time was any retardation of these layers perceived.

In the second place an examination of the working formula shows that as the difference between the times of the falling clouds with battery off and on becomes larger and larger, any error in the timing of the gravity velocity, assuming the other to be constant, has a smaller and smaller effect upon the calculated value of e.

This is perhaps most clearly shown by means of simple arithmetical illustration. Let us consider the last determination in the series of data for doubles given below. Here the average time for "field off" was 12 seconds and for "field on" 8.53 seconds. The calculated value of the charge is 9.084×10^{-10} E.S. units. Now, if the average time for "field off" had been 12.4 seconds in place of 12 seconds, the calculated charge would have been 9.60×10^{-10} E.S. units, which differs from the first value by 5 per cent. Let us suppose now that the average time for "field off" is 12 seconds as before and for "field on" is 10 seconds. This would give a calculated charge of 4.47×10^{-10} E.S. units, which is approxi-

Field.	V_0	V	R	P	$2e$
2,465 volts.	.0444 cm.	.0645 cm.	.200 cm.	.550 cm.	9.50×10^{-10} E.S.
2,465	.0425	.0606	.200	.550	8.378
2,465	.0441	.0639	.200	.550	9.335
2,465	.0462	.0667	.200	.550	9.886
2,465	.0449	.0667	.200	.550	10.340
2,465	.0476	.0667	.200	.550	9.352
1,605	.0303	.0454	.200	.550	9.113
2,265	.0385	.0506	.400	.910	9.592
2,265	.0348	.0476	.400	.910	9.649
2,265	.0345	.0471	.400	.910	9.446
2,265	.0296	.0430	.400	.910	9.322
2,365	.0308	.0445	.400	.860	8.792
2,365	.0308	.0455	.400	.860	9.411
2,365	.0292	.0435	.400	.860	9.136
2,365	.0347	.0488	.400	.860	9.609
2,365	.0333	.0469	.400	.860	9.084

Mean $2e = 9.372 \times 10^{-10}$ E.S.

Hence $e = 4.686 \times 10^{-10}$ E.S.

mately a single. If in this case, however, the average time for "field off" is changed to 12.4 seconds, the battery velocity remaining the same, the calculated charge would be 5.06×10^{-10} E.S. units, or a difference of 13 per cent., which is more than twice as much as when the timings gave a double charge calculation.

Frequently when the rate of expansion was slow, fast clouds comprised of large drops carrying multiple charges were obtained. A number of observations were taken under such conditions and are given below. In determining e from these the calculated charge is assumed to be some multiple of the average value obtained for e in the preceding determinations by singles and doubles.

Field.	V_0	V	R	P	ne	e
volts.	cm.	cm.	cm.	cm.		
2,365	.0496	.0667	.400	.86	13.88×10^{-10} E.S.	4.63×10^{-10} E.S.
2,465	.0503	.0779	.200	.55	13.86	4.622
2,156	.0571	.0860	.400	.86	27.76	4.626
2,165	.0580	.0727	.400	.86	14.16	4.72

Mean $e = 4.65\times10^{-10}$ E.S.

Giving equal weight to all these determinations the general average becomes:

e from singles	$=4{,}669\times10^{-10}$ E.S.
e from doubles	$=4{,}686$
e from multiples	$=4{,}650$
Final mean e	$=4{,}668$

HEAT TRANSFER DUE TO STEAM CONDENSATION.

By S. Leroy Brown.

A VERY important irreversible process which takes place in a steam engine is the to-and-fro exchange of heat between the working fluid and the cylinder walls as the working fluid rises and falls in temperature. This process is called *cylinder condensation* because actual condensation of steam is a very prominent part of the process; and it has a great influence on the efficiency of a steam engine. Some writers on the steam engine describe this effect as if it were a genuine heat conduction from the hot steam to the cool cylinder walls at one stage of the steam-engine cycle and from the cylinder walls to the cooler steam during another stage of the steam-engine cycle. It is generally recognized, however, that this to-and-fro exchange of heat depends very largely upon the condensation of the steam upon the cylinder walls and its subsequent re-evaporation, and the use of a high-temperature steam jacket around the cylinder, or the employment of superheated steam reduces the to-and-fro exchange of heat by eliminating actual condensation. Under these conditions the only exchange of heat is that which is due to the thermal conductivity of the steam and cylinder walls in the narrow sense of that term.

It is the purpose of this paper to compare experimentally the rates of heat flow from steam to cold metal under two conditions as follows: (*a*) When condensation takes place and (*b*) when no condensation takes place. For the purpose of this comparison hot air is assumed to have the same thermal conductivity as superheated steam, and the comparison is made between the rate of heat flow due to the condensation of steam upon a cold metal surface, and the rate of heat flow when hot air is in contact with the cold metal surface. That is to say, the rates of heat flow are determined (*a*) when a cold metal is immersed in steam and (*b*) when a cold metal is immersed in hot air at the same temperature as the steam.

A solid brass ball weighing 250 grams was placed in steam and in hot air for periods ranging from 20 to 300 seconds and the amount of heat absorbed by the brass for each period of exposure was determined by a calorimeter. The hot air bath was produced by adjusting a Bunsen flame under a large galvanized iron vessel about 20 centimeters high and 10 centimeters in diameter. A thermometer was placed with its bulb near the point in the vessel where the brass ball was to be exposed and by carefully stirring the air inside the vessel the desired temperature could be maintained. When a steam-bath was used instead of hot air some water was put in the vessel and the remainder of the procedure was exactly the same as in the case with the air-bath.

In order to determine how much steam was condensed by the cool brass, a small thin copper cup was suspended below the ball to catch the water which would drop from the ball. The ball and cup were weighed before and after exposure to the steam and thus the weight of condensed water was measured. The copper cup was heated to the temperature of the steam and attached to the cold ball just before they were lowered into the steam and thus any error that might be produced by steam condensing on the copper cup was eliminated. The ball after removal from the steam would reëvaporate some of the water clinging to it if allowed to remain exposed to the atmosphere during the weighing. This loss of condensed water was avoided by quickly dropping the ball and cup into a

TABLE I.

Brass in Hot Air at 94° C.

Time Exposed (Sec.).	Temp. of Hot Air (° C.).	Temp. of Brass Before Immersion (° C.).	Heat Absorbed (Calories).	Mean Heat Absorbed (Calories).
20	94	20.5	29.5	
20	93.5	20.7	28.5	
20	94	20.7	31.7	29.7
60	94.5	20.5	90.5	
60	94	20.5	92.9	91.7
120	94	20	161.8	
120	93.5	22	163	
120	93.5	20	163	162.6
300	94	20.5	462	
300	94	18	406.9	433.5

small beaker of water and measuring the increase in weight of the ball, cup, beaker and water.

TABLE II.

Brass in Steam at 94° C.

Time Exposed (Sec.).	Temp. of Steam (° C.).	Temp. of Brass Before Immersion (° C.).	Heat Absorbed (Calories).	Mean Heat Absorbed (Calories).
20	96	22.2	698	
20	94	20	712.6	
20	94	20.5	669.7	693.4
60	93.5	22.2	1,463.2	
60	93 5.	24	1,499.2	
60	93	22.2	1,484.7	1482.4
120	93	24	1,548	
120	94	24	1,598	1573
300	94	22.2	1,580	
300	94	22.2	1,616	1598.2

TABLE III.

Brass in Steam at 94° C.

Time Exposed (Sec.).	Temp. of Steam (° C.).	Temp of Brass Before Immersion (° C.).	Grams of Steam Condensed.	Mean Grams of Steam Condensed.	Heat Liberated (Calories).
20	94	22.1	1.2		
20	94	22.3	1.22		
20	94	21.8	1.21		
20	93.5	22	1.22	1.2125	649
60	94	22	2.62		
60	94	22.2	2.61		
60	94	20.8	2.62	2.616	1400
120	93.5	21.6	2.55		
120	94	22.2	2.53	2.54	1362
300	94	22	2.44		
300	93.5	21.8	2.45	2.445	1310

The accompanying tables and curves show the results of such observations and calculations. The ordinates of curve *A*, Fig. 1, represent the heat absorbed (in calories) and the abscissas represent the time (in seconds) that the brass ball was exposed to hot air. Curve *B* is the relation between the same two quantities when the ball was exposed to steam. The temperatures of the steam and

hot air were as nearly as possible the same (94° C.) and the temperature of the ball before being heated was about 20° C. The ordinates of the curve *C* represent the heat liberated by the measured condensed steam and the abscissas represent the time that the ball was exposed to the steam; the heat liberated by condensation being calculated from the weight of the steam condensed and the heat of vaporization of water.

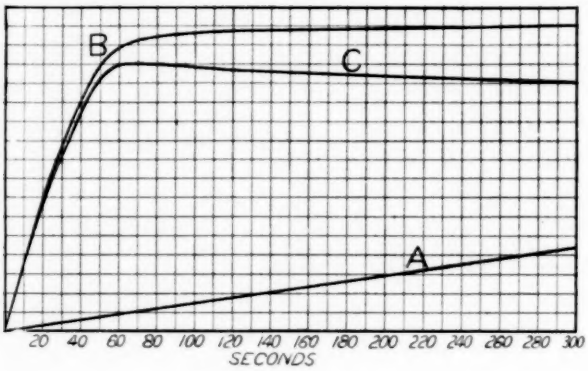

Fig. 1.

The near approach of curve *C* to curve *B* throughout a large portion of their range shows that nearly all the heat given to the ball can be accounted for by the heat of vaporization of the condensed steam. The small slope of curves *B* and *C* beyond a period of exposure greater than 60 seconds is on account of the ball nearly reaching the temperature of the steam at this time.

The temperature of the ball rises so quickly when it is in steam, that the effect of the condensed steam must be determined by comparing the rates of absorbing heat when the periods of exposure to steam and to hot air are short. The ratio of corresponding ordinates of curves *B* and *A* at an abscissa of 20° is about 24 which means that the heat is conducted to a cold metal 24 times as fast from steam at 94° C. as from hot air at the same temperature.

The data of Tables IV., V., VI., VII., VIII. and IX. were taken similarly to that of Tables I., II. and III., but the temperature of the bath from which the metal absorbed heat was higher. Curves

TABLE IV.

Brass in Steam at 98° C.

Time Exposed (Sec.).	Temp. of Brass Before Immersion (° C.).	Heat Absorbed (Calories).
10	23	729
10	33.7	767
20	24	1,101
20	24.2	1,030
40	24	1,375
60	24.2	1,536
120	25	1,576
180	24.2	1,595
300	24.2	1,590

TABLE V.

Steel in Steam at 98° C.

Temp. of Steel Before Immersion (° C.).	Heat Absorbed (Calories).
24.2	741
24	1,095
24	1,635
25	1,798
24.2	1,811
24.2	1,850
24.2	1,870

TABLE VI.

Brass in Hot Air at 98° C.

Time Exposed (Sec.).	Temp. of Brass Before Immersion (° C.).	Heat Absorbed (Calories).
20	24	36
20	24	28
60	24	93
180	24	242
300	24	425

TABLE VII.

Steel in Hot Air at 98° C.

Temp. of Steel Before Immersion (° C.).	Heat Absorbed (Calories).
24	27
24	32
24	69
24	192
24	312

TABLE VIII.

Brass in Steam at 99° C.

Time Exposed (Sec.).	Temp. of Brass Before Immersion (° C.).	Heat Absorbed (Calories).
20	24	1,373
40	24	1,536
60	24	1,586
180	24	1,577
300	24	1,600

TABLE IX.

Steel in Steam at 99° C.

Temp. of Steel Before Immersion (° C.).	Heat Absorbed (Calories).
24	1,569
24	1,800
24	1,879
24	1,912
24	1,923

D and *E* show the relation of heat absorbed by a brass ball from a steam-bath to the time of exposure of the ball to steam at temperatures of 98° and 99° C. respectively. Curves *F* and *G* show the relation between the same two quantities for a steel ball exposed to

steam at temperatures of 98° and 99° C. respectively. At the higher temperature less air is mixed with the steam and the higher rate of heat absorption from the pure steam is noted from the divergence of curves *D* and *E* or *F* and *G*.

Curves *H* and *I* show the rates at which the brass and steel balls, respectively, absorb heat from hot air. The ratio of ordinates of curves *E* and *H* at an abscissa of 20 seconds shows that the

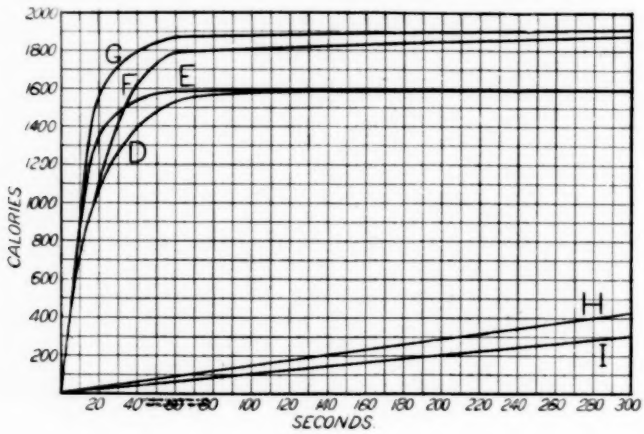

Fig. 2.

brass ball absorbs heat about forty times as fast from steam at 99° C. as from hot air at the same temperature. The ratio of ordinates of curves *G* and *I* at an abscissa of 20 seconds shows that the steel ball absorbs heat about fifty times as fast from steam at 99° C. as from hot air at the same temperature.

CONCLUSION.

The rate of delivery of heat by hot steam to a cold metal surface is at least forty times as great as the rate of delivery of heat to the cold metal surface by hot air at the same temperature as the steam, and therefore assuming superheated steam to be approximately the same as air in its thermal conductivity it is evident that the elimination of actual condensation on the cylinder walls of a steam-engine greatly reduces the exchange of heat between the working fluid and the cylinder walls.

LEHIGH UNIVERSITY,
February 15, 1910.

THE REFLECTING POWER OF ICE IN THE EXTREME INFRA-RED SPECTRUM.

By A. Trowbridge and B. J. Spence.

AMONG transparent bodies water is probably the least diathermanous substance of definite chemical constitution obtainable in a reasonably pure state. It is also exceptional in that it possesses an abnormally high dielectric constant.

The dispersion of water has been determined in the ultra-violet by Simon[1] in the visible spectrum by Flatow,[2] and in the nearer infra-red spectrum by Rubens.[3] The constants of the dispersion formula of the Sellmier type have been determined by Flatow and the large value of one of these constants indicates strong absorption in the remote infra-red spectrum. It is possible that the strong absorption may be due to many infra-red bands, or to a single band. If it be assumed that there is a single absorption band in the infra-red it should occur at a place in the spectrum corresponding to a wave-length of 0.076 mm. (76μ).

With the perfection by Rubens and Nichols[4] of the method of isolating by repeated reflection infra-red radiations of great wave-length came the possibility of studying the properties of water in a spectral region relatively near that for which it was calculated that water should selectively reflect. In fact, Rubens and Aschkinass[5] found that long heat waves obtained by multiple reflections from sylvite ($\lambda = 61\mu$) were more strongly absorbed by water vapor than were the residual rays from rock salt. This would make it seem that water should have a region of selective absorption somewhere beyond 61μ. However, subsequent experiment by Rubens and Ladenburg,[6] with very thin liquid films were interpreted by

[1]Wied. Ann., 53, 542.
[2]Drude's Ann., 12, 85.
[3]Wied. Ann., 45, 238.
[4]Ann. der Physik und Chemie, 65, 252
[5]Ann. der Physik und Chemie, 65, 252.
[6]Le Radium, VI., and Berlin. Sitzungsberichte, 1908.

the authors to mean that there existed no region of selective absorption in the extreme infra-red spectrum.

Note.—During the progress of this investigation of the properties of ice made by us, there has appeared an important paper by Rubens and Hollnagel[1] in which they report the isolation of two new regions of residual rays and the determination of their mean wave-lengths. From the behavior of water towards these new radiations whose wave-lengths are greater than 76μ, they conclude that water does not show selective reflection in the region in which the dispersion formula predicts that it should occur.

The fact that the dispersion formula has proved in the main correct in predicting the location of the infra-red absorption bands of quartz, fluorite, rock salt and sylvite is strong presumptive evidence in its favor and it has therefore seemed to us of importance to endeavor to establish the existence or non-existence of selective reflection from water by applying the most direct experimental method available.

Were it not for the presence of water vapor over the free surface of the liquid, the most direct method of attack would be to cause a beam of light to be repeatedly reflected from a water surface, whereby the beam would be deprived of the shorter wave-lengths and thus consist chiefly of those wave-lengths selectively reflected. Inasmuch as the presence of water vapor would give inconclusive results, it was necessary to devise another method.

The method used in the investigation was to allow the successive reflections to take place from polished ice surfaces, which, together with the source of light and bolometric apparatus, were kept in a room whose temperature could be maintained considerably below the freezing point.

The justification for such a method lies in the fact that, optically, ice and water are very nearly identical, in the visible and nearer infra-red regions of the spectrum, and hence the assumption is probably warranted that a possible region of selective reflection of one would coincide with that of the other. This matter is, however, more fully discussed in the concluding paragraphs of the article.

The arrangement of the apparatus was briefly as follows: An

[1]Berlin. Sitzungsberichte, 1910.

electric arc, the positive crater of which served as a source of radiation, was placed at the principal focus of a silvered concave spherical mirror. The parallel beam of radiation thus furnished was allowed to reflect successively from three large plane surfaces of polished ice, and a second concave spherical mirror, mounted on a base which could be rotated about a vertical axis, brought to focus on the bolometer strip all possible residual radiation reflected by the three ice surfaces. A shutter was interposed between the arc and first concave mirror, in order that the beam of radiation might be shut off or allowed to pass at will. The whole apparatus was then properly shielded to protect it from stray or scattered radiation.

The bolometer was mounted in an air-tight brass case provided with a window of quartz one millimeter thick and cut perpendicular to the optic axis, so as to admit the beam from the second concave mirror. Quartz was chosen as a suitable substance because it has been found by Rubens and Aschkinass[1] to be fairly transparent to radiation of wave-length greater than 45μ, *i. e.*, in the region where it was expected that ice would selectively reflect.

The two bolometer strips were each 1.5 mm. wide and 25 mm. long, and were similar as regards resistance to within one part in two hundred when carrying a current of 0.16 ampere, and had been properly blackened with a fine, coherent soot from burning camphor. The bolometer case was evacuated to a pressure of about 0.5 mm. of mercury, thus avoiding to a large extent irregular charges in resistance due to air currents.

The galvanometer used in conjunction with the bolometer was a four-coil, astatic needle system provided with a triple shield of soft iron. As used in the present instance it had a sensibility of 8×10^{-11} amperes, with a period of 6 seconds and resistance of 3 ohms. The "candle at a meter distance" sensibility was 7,000 mm., *i. e.*, the radiation from a candle at a distance of one meter falling upon one bolometer strip, without concentration by means of mirrors or lenses would give the above deflection at full sensibility. Irregular motions of the galvanometer needle, due to current fluctuations or convection currents due to the residual gas in the bolometer case did not exceed 0.5 mm., though at times there was a slow drift due

[1] *Loc. cit.*

to temperature changes in the room. This room is a central unit of a cold storage system in the laboratory and could be kept indefinitely at a temperature considerably below freezing. The temperature for the present work was kept at $-22°$ C.

In order to adjust the optical system it was necessary to place plane silvered mirrors in contact with the ice surfaces, and after the multiple reflections bring the image of the arc upon the bolometer strip by means of the rotating concave mirror mentioned above. The plane mirrors were then carefully removed from the ice surfaces and the concave mirror, provided with a fine adjustment, was slowly rotated until a maximum deflection of the galvanometer occurred, when the opaque shutter was opened.

This adjustment was repeatedly made with the result that there could be found no position of the concave mirror for which the galvanometer deflection obtained on opening the shutter exceeded 20 mm. This was, as will be shown later, only about one per cent. of the deflection which should have been observed in the event of ice showing selective reflection of a mean wave-length of 76μ.

The radiation whose absorption by the bolometer caused this deflection of 20 mm. passed without marked absorption through plates of glass, rock salt and quartz, and was about completely absorbed by a plate of clear ice, hence showing that this radiation was of short wave-length, and probably represents that fraction of the energy scattered by the ice surfaces which arrived at the bolometer strip in spite of precautions taken to prevent it.

In order to assure ourselves that our apparatus was sufficiently sensitive to detect radiation of a mean wave-length $\lambda = 76\mu$, present in our source of light, we prepared three polished surfaces of fused potassium bromide, which Rubens and Hollnagel have shown to reflect selectively, energy of a mean wave-length, $\lambda = 82.3\mu$, and substituted them for the three ice surfaces.

On performing the necessary adjustments for a maximum deflection it was found necessary to reduce the galvanometer sensibility to 6×10^{-9} amperes, in order that the deflections might not exceed the limits of the scale. As the bolometer current remained the same, this was equivalent to reducing the efficient sensibility in the ratio 1 : 75.

Owing to the fact that only three potassium bromide surfaces were used and because this polish was by no means as perfect as that of the ice surfaces, the residual radiation was probably not homogeneous, and we therefore substituted for the opaque metallic shutter of the former experiment, one of rock salt, which allowed diffusely reflected energy of shorter wave-lengths continually to reach the bolometer strip. Under these conditions the deflection obtained on opening the rock salt shutter was for the most part due to residual radiation greater than 45μ, and undoubtedly of mean wave-length 82.3μ, which potassium bromide selectively reflects. The average deflection thus obtained was 24 mm., which when multiplied by the factor 75, to give full sensibility conditions as used in the case of ice, gave a deflection of 1,800 mm., which represented energy of mean wave-length 82.3μ. Of this amount practically nothing was reflected from the ice, assuming that in case ice did selectively reflect, the deflection of the galvanometer would have been of about the same magnitude as that from potassium bromide surfaces, and hence we are confident that ice shows no selective reflection in the region from $\lambda = 45\mu$ to $\lambda = 82.3\mu$.

It is however necessary to be cautious in drawing conclusions from this fact, and applying them to water, in the remote infra-red spectrum, for even though the optical properties of ice and water appear to be almost identical, there is however another property in which they do not seem to be identical, namely, the dielectric constant. Were this not true, there would be strong presumptive evidence of ice and water possessing similar properties in the remote infra-red.

Unfortunately, the specific inductive capacity of ice has not been determined with any degree of certainty. Bouty,[1] Perot[2] and Rosa[3] report the values 78, 60 to 71, and 75.7 respectively. Thwing,[4] Abbey,[5] and Behm and Kubetz[6] have obtained the values 2.85, 3.16 and 1.76 to 1.88 respectively.

[1]C. R., 114, 533.
[2]C. R., 114, 1528.
[3]Phil. Mag., XXXI., 188.
[4]Zeit. Phys. Chem., 14, 286.
[5]Wied. Ann., 65, 229.
[6]Boltzmann's Festschrift, 1904.

Mr. P. Thomas, of the department of electrical engineering of Princeton, has kindly permitted us to report values which he has recently obtained by an alternating current method, the details of which will be published shortly. The mean value of the specific inductive capacity of ice obtained from sixteen observations under various conditions of frequency is 83.5. The individual values of which this is the mean, lie in the range from 70 to 90.

In order to determine where the dispersion formula of the Sellmeier type

$$n^2 = a + \frac{D_v\lambda^2}{\lambda^2 - \lambda_v^2} - e\lambda_v^2$$

predicts the existence of selective reflection from ice, we have calculated the constants approximately satisfying the experimental values of the dispersion obtained by Pulfrich[1] and Meyer.[2] The values of the constants are $\lambda_v^2 = .0158$, $D_v = .427$, $a = 1.28$ and $e = .0133$. The values of the constants are perhaps the best values available, for the range of wave-lengths over which the dispersion of ice has been determined is very limited, extending from the B line ($\lambda = 6867$ A.U.) to the F line ($\lambda = 4861$ A.U.), and it became difficult to fit the dispersion formula with constants, which will predict absorption bands to any degree of accuracy in the remote infra-red spectrum. Nevertheless, if we use the above constants and the value of the dielectric constant as 80, then

$$\lambda_r = \sqrt{\frac{\epsilon - (o_v + a)}{e}} = 76.5\mu,$$

and if the value 2.85 is used as dielectric constant, the formula predicts a band at 2.9μ. The use of the high value for the dielectric constant for ice locates the band in the same position as that predicted for water, at 76μ. Very little importance is attached to the value 2.9μ, located by the use of the value 2.85 as a dielectric constant, owing to the fact that observers who have reported low values for the dielectric constants have reported values which vary over a large range, and this, of course, adds a decided uncertainty to the location of the band.

[1]Wied. Ann., 34, 339.
[2]Wied. Ann., 31, 321.

As a brief summary of the experiment it may be said that ice shows no region of metallic reflection in the infra-red spectrum in the interval from $\lambda = 45\mu$ to 82.3μ. This probably can be said of water, yet in the present state of uncertainty as to the true value of the dielectric constant of ice it is impossible to state as to the validity of the dispersion formula as applied to ice, and it is likewise impossible to draw trustworthy conclusions from the optical properties of ice, and apply them to the same substance in the liquid state.

PALMER PHYSICAL LABORATORY,
PRINCETON, N. J.,
March, 1910.

AN INVESTIGATION OF NON-VIBRATORY TYPES OF FREQUENCY METERS.

BY C. E. HIATT.

FLEMING[1] and Pierce[2] have mentioned the fact that the deflecting moment acting on the suspended ring of the former's A.C. galvanometer is a function of the frequency. No experimental data showing the performance of this galvanometer when used as a frequency indicator have been found after a careful examination of the literature.

It is the object of this paper first to describe a modification of this instrument for the purpose of frequency determination and to give an account of its performance; secondly to discuss in a similar fashion a bolometric frequency meter devised by the writer. This was suggested by a paper on kinetic hysteresis by Guye and Hertzfeld,[3] reference to which was made in a previous paper.[4] Inasmuch as this paper is practically a continuation of the one just mentioned, details given in the former will be omitted in this.

PART I.

Fig. 1 is a diagrammatic sketch of the first instrument referred to and is self explanatory. To call it a modified form of the Fleming galvanometer may seem at first to be in error, for the instrument is just as truly a core type transformer with a secondary free to turn about an axis perpendicular to the plane of the core. The reaction between the field due to the current in this short-circuited

[1] Fleming, The Principles of Electric Wave Telegraphy, Section 13, p. 150, and 12, p. 397. (As a matter of historical interest it may be mentioned that the invention of the Fleming A.C. galvanometer bears an uncertain date. On page 150 of the above reference Fleming says he devised his A.C. galvanometer in 1884 and on p. 397 of the same treatise he says he devised the instrument in 1887.)

[2] G. W. Pierce, Experiments on Resonance in Wireless Telegraphy Circuits, PHYS. REV., Sept., 1904, Vol. 19, p. 201.

[3] Guye and Hertzfeld, C. R., 136, p. 957, 1903.

[4] Hiatt, PHYS. REV., Vol. 29, No. 5, p. 432, 1909.

secondary and the inducing field gives the turning moment. The instrument is, however, still essentially a Fleming galvanometer, the Fleming instrument being merely an application of an air core alternating current transformer with a short-circuited and movable secondary. The one under discussion differs from it merely in the fact that it has an iron core.

The disc forming the short-circuited secondary is a copper washer 2 mm. thick and 2.38 cm. in diameter with a 0.79 cm. hole in the center. The inner edge is slightly beveled for a reason which will appear shortly. From this disc there extends a copper wire to support a mirror mounted at 45° to the plane of the disc. This copper wire also extends as shown in the figure to a disc immersed

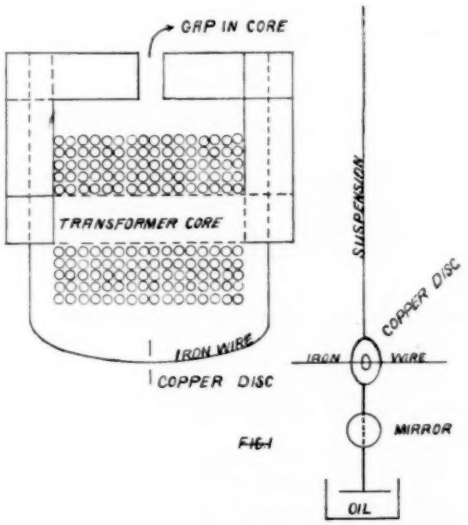

Fig. 1.

in oil which serves as a damper. The suspension is of phosphor bronze 0.025 mm. thick and 0.43 mm. wide. The copper disc is threaded on an iron wire 1.27 mm. in thickness; the beveling of the inner edge permits it to swing freely through a large angle. The iron wire is between the central laminations of the transformer core. These laminations are of silicon steel 0.356 mm. thick and form a core 2.54 cm. by 5.08 cm. in cross section. The sides of the core rectangle are 15.24 cm. and 10.16 cm. respectively; the air

gap is 0.60 cm. in length. The winding consists of two coils having 900 turns each of double cotton covered copper wire 1.62 mm. in diameter connected in multiple.

An inverted Westinghouse rotary converter run by storage cells furnished the alternating current for the transformer. By changing

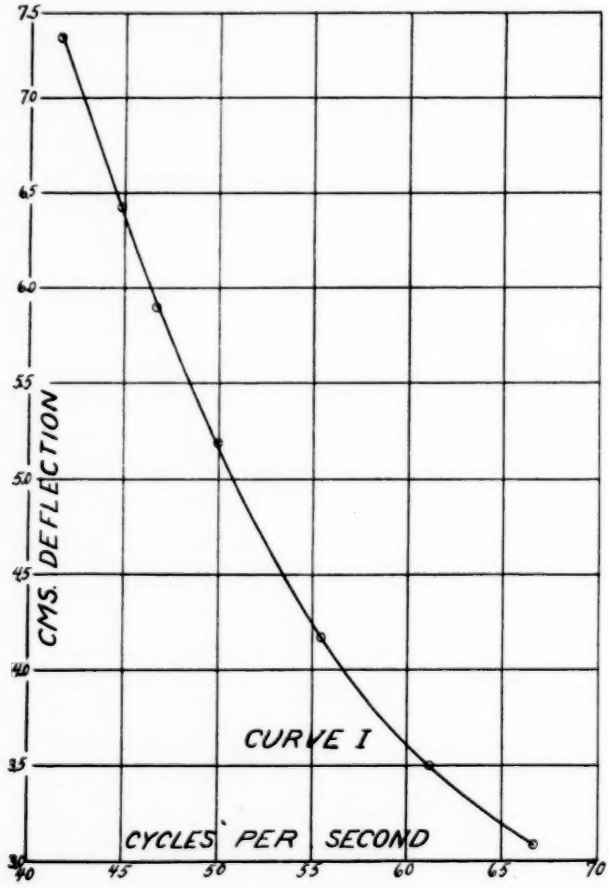

Fig. 2.

the field excitation a fair range of frequencies was obtained. The alternating voltage supplied to the instrument was kept constant by means of a multicellular electrostatic voltmeter across the A.C. mains and a rheostat in the D.C. supply. The frequency was determined with a Starrett revolution counter and stop watch, the

shaft of the revolution counter being connected to the shaft of the alternator by a short piece of high-grade rubber tubing.

The copper disc was initially set with its plane at approximately 45° to the iron wire, the 45° position being the one for maximum torque on the disc. Any deflection of the disc from this position was observed by means of the attached mirror and a telescope and scale. The scale distance was one meter.

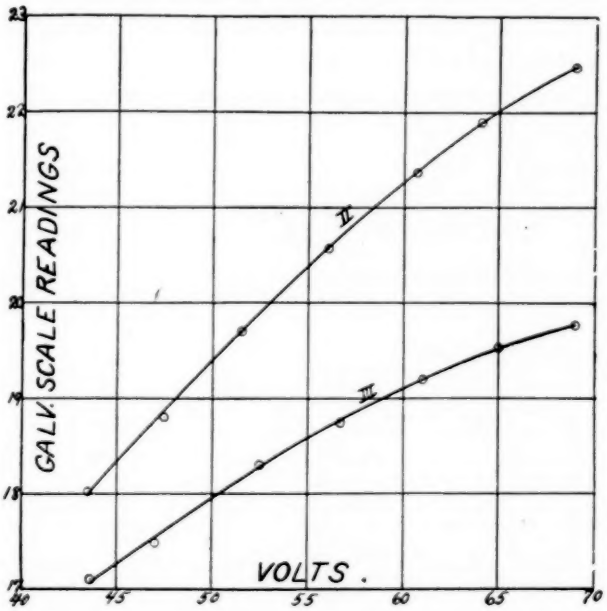

Fig. 3.

Curve *I*, plotted from the data of Table I., shows the performance of the instrument when the supply voltage was kept constant at 60 volts.

TABLE I.

Frequency.	Deflection.
41.7	7.32
43.8	6.43
46.7	5.90
50.0	5.19
55.5	4.18
61.7	3.50
66.7	3.08

TABLE II.

Volts.	Readings.
43.5	18.02
47.5	18.80
51.5	19.70
56.0	20.56
60.6	21.90
69.0	22.48

TABLE III.

Volts.	Readings.
43.6	17.1
47.0	17.50
52.5	18.30
56.6	18.75
61.0	19.20
65.0	19.52
69.0	19.78

For the lower frequencies it is quite sensitive, but beyond this and the fact that it is easy and cheap to construct, it has little, if anything, to commend it. To get a high torque and high sensibility it is necessary to use for the disc a low resistance metal with its accompanying high temperature coefficient. Furthermore, the indciations of the instrument are seriously dependent upon the variation of wave form and voltage. This dependence upon the voltage may be seen from curves *II* and *III* plotted from the data of tables II. and III. which have been placed by the side of Table I. Curves *II* and *III* were obtained by keeping the frequency constant at 48 and 60 cycles per second respectively and varying the voltage by means of a non-inductive resistance in series with the instrument. The voltage given is that across the transformer terminals and was read from a Weston A.C. voltmeter of the dynamometer type. The dependence upon variation of wave form may be inferred from the results of numerous experimenters, those of Lloyd in the various Bulletins of the Bureau of Standards being perhaps in the most complete and available form.

PART II.

The writer's bolometric frequency meter which depends for its action on the rise in temperature of a thin iron wire subjected to cyclic reversals of magnetic flux seems to be a more satisfactory instrument. Its range is far more extended; in fact, there is reason for the belief that a single instrument can now be made which will measure any frequency from 10 to 10^6 cycles per second with a very fair degree of accuracy. The bolometric type of instrument has some decided advantages over the thermo-hysteretic frequency meter described by the writer in the article already referred to. This last type of meter is dependent on the production of hysteretic heat at a thermo-junction. In brief the advantages just mentioned are exactly those which the resistance thermometer has over the thermo-couple thermometer. The former with its bridge arrangement permits the use of sturdy indicating instruments while the thermo-couple demands a highly sensitive galvanometer. In the case of the resistance thermometer much larger quantities of energy are available for measurement. The thermo-couple type of instru-

ment is compact and easy to construct, but beyond this the more elaborate and expensive bridge arrangement seems preferable. Having made a frequency meter on the thermo-couple principle, it was merely a logical development to construct the more elaborate bolometric type.

The instrument and testing equipment are shown diagrammatically in Fig. 4. The iron wire bridge of which A, B, C and D are

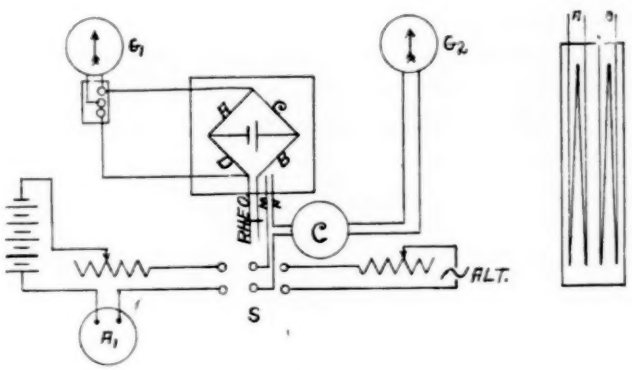

Fig. 4.

the arms is enclosed in a tight box of insulating material which shields it from external temperature changes. Two opposite arms of this bridge are mounted on thin rectangular strips of mica as shown on the right side of the figure. Each arm consists of an iron wire 0.0513 mm. in diameter and about half a meter long, strung zig-zag fashion through holes in the mica strips. The two rows of holes in the mica are about 4.6 cm. apart. As adjacent lengths of wire are separated by the mica sheet, short circuiting of portions of the wire is effectively prevented. The stringing of such fine, springy wire on such a frame is a rather delicate and tedious process. Two frames are made containing the arms A and B and C and D respectively. C and D are then placed in a solenoid of 1,740 turns of double silk-covered copper wire 0.508 mm. in diameter and wound on a wooden spool. A and B are placed in a solenoid containing the same amount of wire and exactly like the first except that it is wound non-inductively. These two solenoids are permanently connected in series and attached to the wires M and N coming from the switch S, which furnishes the exciting current. When

alternating current is supplied to these solenoids the bridge arms C and D get hotter than A and B and due to the high temperature coefficient of iron, the bridge becomes unbalanced. This is indicated by the mirror galvanometer G_1. The current in the coils is kept constant by means of a variable resistance and the thermo-comparator C used in conjunction with the galvanometer G_2 to form a thermo-ammeter.[1] By means of this arrangement the current used was kept constant to 0.2 per cent. or better.

In preparation for a test of the instrument direct current of the value to be used was taken from the storage cells shown at the left of the figure. This was passed through the coils for some time in order that a steady temperature state might be had in the box,

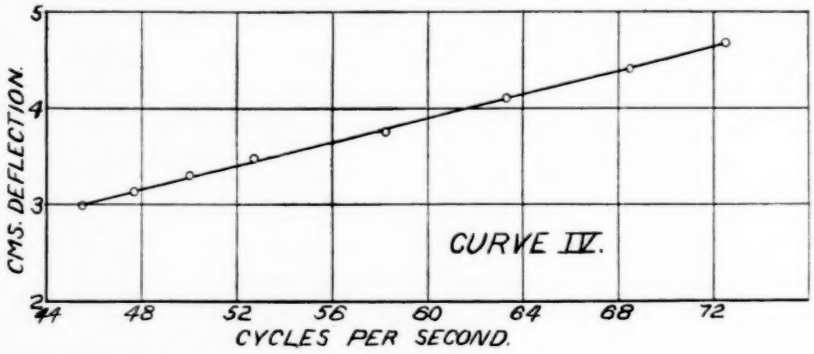

Fig. 5.

thus eliminating possible thermo-electric disturbances due to a sudden heating of the coils and bridge arms when measurements were begun. This steady current with the ammeter A_1 served also to calibrate the thermo-ammeter. The lower corner of the bridge rectangle is shown equipped with a slide wire rheostat. This was used to secure an initial balance of the bridge. For the low frequencies this initial balance was taken with direct current in the coils, but for the high frequency obtained from the inductor alternator it was convenient to balance at a frequency of about 800 cycles per second and observe deflections at the higher frequencies. The Ayrton shunt connected to G_1 proved very convenient.

Curves *IV* and *V*, Figures 5 and 6, show the performance of the

[1] Hiatt, PHYS. REV., Vol. 29, No. 5, p. 441.

instrument under two different ranges of frequency. They are plotted from the data of Tables IV. and V. respectively. The frequencies of Table IV. were obtained just as those of Table I.; those of Table V. from the inductor alternator driven by a high speed motor also supplied from the storage cells.

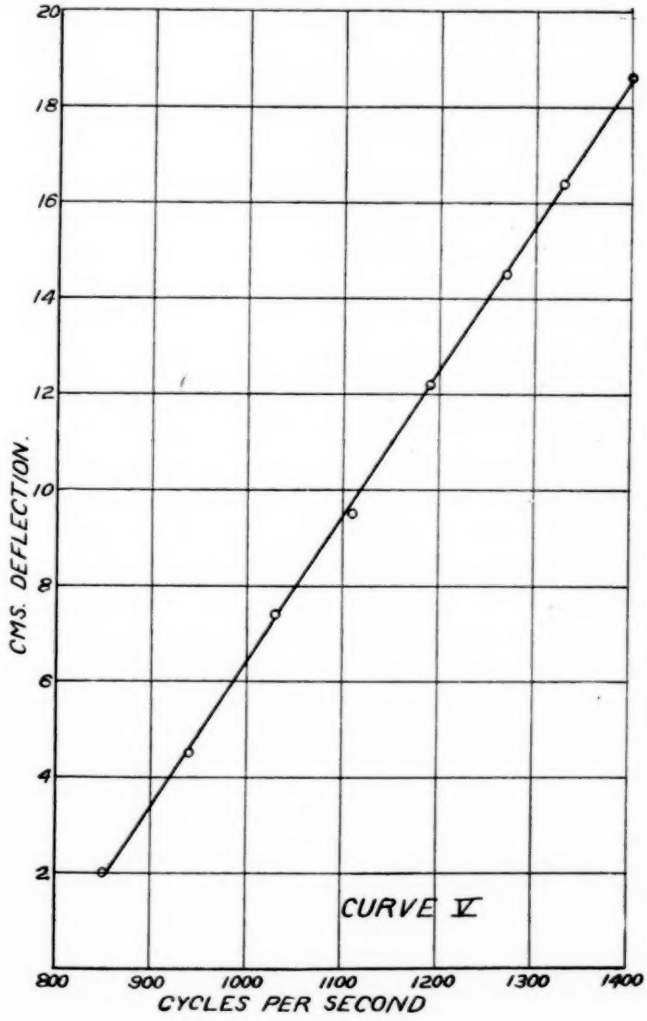

Fig. 6.

For Table IV. the current through the two solenoids was 0.09 ampere and for Table V. 0.07 ampere.

TABLE IV.

Frequency.	Deflection.
45.5	2.98
47.7	3.12
50.0	3.30
52.7	3.48
58.2	3.75
63.3	4.10
68.5	4.42
72.5	4.68

TABLE V.

Frequency.	Deflection.
850	2.0
940	4.5
1,030	7.4
1,110	9.5
1,190	12.2
1,270	14.5
1,330	16.4
1,400	18.6

This first instrument can be improved by the use of finer iron wire in the bridge arms to eliminate an annoying sluggishness of action. As iron wire of almost one fourth the diameter of that used in this work can now be obtained of Hartmann & Braun, it is believed that the objection just stated may be eliminated. It is the writer's intention to try an improved mounting for the bridge arms similar in general to that of a tungsten lamp filament. A variation in wave form of the exciting current must cause variations in the readings of the bolometric frequency meter just as in the case of the thermo-hysteretic device and the Fleming galvanometer. This necessitates empirical calibration. The most valuable feature about the instrument is its high sensibility, even in its present unrefined state.

I take pleasure in acknowledging my indebtedness to Professors A. W. Goodspeed and H. C. Richards for their interest and encouragement and to the former for placing the necessary equipment at my disposal. I also wish to thank Messrs. F. Kalmbach and J. Martin, to whom I am indebted for assistance with the mechanical details.

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A LABORATORY ARC LAMP.

By Edwin F. Northrup.

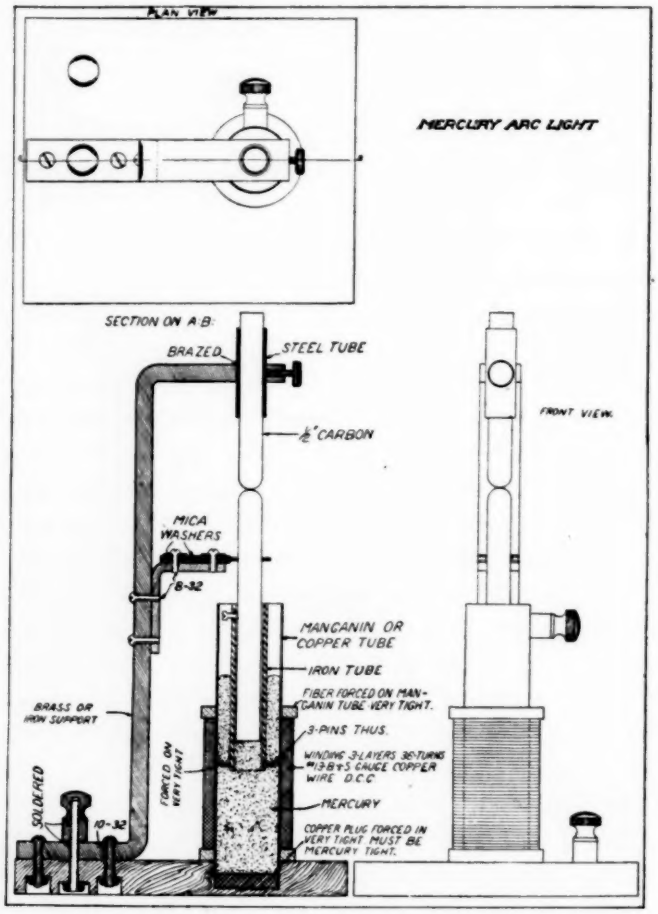
THE writer has had occasion to make use of an arc light, in some photographic work, which was required to give a highly actinic light and operate with steadiness. He devised for this purpose the form of self-regulating arc light shown in the cut. As the apparatus is very inexpensive to construct and operates in a very perfect manner for laboratory purposes, he has considered that a description of the arc light will not be out of place, as it may prove useful to others.

The figure is almost self-explanatory as to the form of construction. The upper electrode can be either carbon or steel and has a vertical adjustment. The lower electrode which can move vertically is clamped in an iron tube which floats in a well of mercury. Around the well of mercury is a solenoid of three layers, of thirty-six turns each, of no. 13 B. & S. gauge copper wire.

The current enters the upper electrode, goes in series through the two electrodes into the mercury, through the solenoid and out to the negative binding post. The arc may be operated on a 110-volt circuit with a very small resistance in series. As soon as the current starts, the electrodes are drawn apart by the action of the solenoid on the iron tube in which the lower electrode is held and the arc is maintained with extraordinary steadiness. The best material to use for the well holding the mercury is a tube of manganin. This is not acted upon by mercury and it is easy to maintain the well free from leaks. If the upper electrode is made of steel, and made positive, the light of the arc has a bluish cast and has very strong actinic properties.

It is dangerous to look at this iron arc without wearing colored glasses. Four hours after looking into the arc, the writer's eyes became badly inflamed and remained so for a couple of days, no indication being experienced until the inflammation started in, that the eyes were strained.

If the two electrodes are made of carbon, the arc will be found very convenient for use in welding together platinum wires, or other experiments where the temperature of the arc is needed.



This apparatus is recommended by its extreme simplicity and the great steadiness of the arc which it maintains, as an inexpensive laboratory form of arc lamp.

PROCEEDINGS

OF THE

AMERICAN PHYSICAL SOCIETY.

THE DEBT OF PHYSICS TO METAPHYSICS.[1]

BY HENRY CREW.

IF I venture to address this society upon a subject where I am liable, perhaps even likely, to be misunderstood, please bear in mind that I do so only in the belief that it is a matter of no small importance for workers in any one science to realize fully the limitations as well as the powers of their own science. It is hardly necessary to add, that while I shall consider a phase of physics which has little to do with experiment, I am not for an instant unmindful of the fact that ours *is* an experimental science, and that all the really great achievements in physics have been wrought through, or have led up to, or have been completed by, experiment and observation. This remark is doubtless true even of the supreme work of Newton, Fresnel and Maxwell. Nor am I forgetful that in days gone by the normal development of sound physics has been much retarded by metaphysics.

Second to none in my admiration of the man who has contributed even a single experimental fact to either the foundation or superstructure of the edifice which we call modern physics, I invite your attention for a few moments to the debit side of the account as it stands between the physicist and metaphysician. This I do with no little trepidation, remembering how easily one may, even with the utmost good will, go astray in a strange field of thought. Metaphysics is a term employed with such a variety of meanings that I must, at the very outset, explain the one sense in which I am using it. I am *not* employing it to indicate "the sum of all knowledge" (Paulsen), or as a synonym for the "science of the absolute" (Hegel), but rather as a branch of philosophy which is, in a certain sense, supplementary to all the individual sciences of phenomena. The metaphysician here in mind is a gleaner after physics and psychology, using these two words in their wide meaning so as to cover practically the whole of modern science. He it is who

[1]Presidential address before the American Physical Society, delivered at the New York meeting, March 5, 1910.

orients the sciences among themselves, criticizes their foundations, their methods and even their conclusions, in so far as these conclusions depend upon pure logic. He it is who rounds out and corrects the individual sciences. It will thus be seen that metaphysics, with these limitations, does not differ widely from the modern usage of the word philosophy; for the metaphysics I have in mind has been aptly characterized as "the supreme science of order."[1] There are those, and I myself am one of this class, who prefer to use the word "epistemology" to describe a metaphysics of this type. It is certainly not the type of metaphysics which allowed Kant to define matter in terms of force.[2] And, in any event, I trust we shall all agree that we are not getting into what Maxwell called "the den of the metaphysician, strewed with the remains of former explorers, and abhorred by every man of science."

But we must be careful to remember that the metaphysician which Maxwell here has in mind is not an epistomologist, but a man of the Hegelian type. Helmholtz[3] boasted that he never lost an opportunity to impress upon his students the principle that "a metaphysical conclusion is either a false conclusion or a concealed experimental conclusion." How far removed the more genuine metaphysics of to-day is from that which held sway during the first half of the nineteenth century and which exasperated men of the type of Maxwell and Helmholtz, may be indicated by the following paragraph from Professor A. E. Taylor,[4] of Aberdeen, himself a distinguished metaphysician. He says:

"Just because of the abscence from metaphysics itself of all empirical premises, it can be no business of the metaphysician to determine what the course of events will be or to prescribe to the sciences what methods and hypotheses they shall employ in the work of such determination. Within these sciences any and every hypothesis is sufficiently justified, whatever its nature, so long as it enables us more efficiently than any other to perform the actual task of calculation and prediction. And it was owing to neglect of this caution that the *Naturphilosophie* of the early nineteenth century speedily fell into a disrepute fully merited by its ignorant presumption. As regards the physical sciences, the metaphysician has indeed by this time probably learned his lesson."

It is hardly necessary to add that the type of metaphysics here exposed is not one to which physics owes anything whatever, and is not the one I have in mind during these remarks.

[1]Congress of Arts and Science, St. Louis, 1904, Vol. 1, p. 236.
[2]Hoeffding, History of Modern Philosophy, Vol. 2, p. 69.
[3]Vorträge, Das Denken in der Medicin, p. 34.
[4]Congress of Arts and Science, St. Louis, 1904, Vol. 1, p. 240.

I. The Mechanical Postulate.

The father of the present Duke of Argyll rendered marked service to science in pointing out how wide-spread is the use of physical and natural law. But nowhere in his notable volume, the "Reign of Law," does he indicate what may be called the most fundamental fact connected with the discovery and employment of such law, namely, that the very existence of laws governing natural phenomena is a postulate laid down, consciously or unconsciously, by the investigator. No one, except the later metaphysicians, has convinced us that however tangled the knot of physical facts which we are called upon to explain, the first thing we *assume* is that these phenomena are subject to law. We assume that we are studying a machine which behaves in a definite manner.

This assumption—which we may call the mechanical postulate—is not something to be discovered or verified by experiment, not something whose adoption stamps a man as a materialist, not something which sane men consider, in order to accept or refuse, but something which *all* men adopt as a laboratory convenience, one might better say, a laboratory essential.

Nor is the mechanical postulate one which is confined to physics; but is employed in all the sciences where men are attempting to bring order out of chaos. It is not, therefore, something to be charged up against one in the sense employed when modern physics is said to rob the world of all spontaneity and sentiment, or when science is said to be devoid of poetry. While we treat nature as a machine and while we adopt the mechanical hypothesis as a necessity of productive scholarship let us be very careful however not to allow ourselves to dogmatize to the extent of saying that a machine is all we have.

Is not the physicist under obligations to the philosopher for making this matter perfectly clear?

Apparent deviations from mechanical law lead to some of the most important biological problems. Animate and inanimate matter may appear, at first glance, to belong in two different categories: and so they undoubtedly do as regards many of the superficial phenomena. But conversation with some of the most productive scholars of our country in zoölogical and botanical lines has convinced me that they are practically all working on the assumption that biological phenomena are physical phenomena. These investigators assure us, moreover, that the introduction of an *enteliche* here and there, wherever convenient, would be sufficient to discourage all serious research on life problems. The same point of view is expressed by Münsterberg when, in his classification of knowledge, he places physics and biology together at the very bottom of the group called "physical sciences."

The hatching of an egg is apparently a different process from that of melting ice, although both are accomplished by the application of heat. But to assume anything else than that they are both mechanical processes is merely

to erect a barrier which shall delay the discovery of truth. The study of cytology and artificial parthenogenesis have already gone so far that the discovery of a much more definite connection between life and mechanics would shock the world perhaps even less than did Wöhler's synthesis of urea in 1828.

UNIFORMITY POSTULATE.

There is of late a very distinct change of feeling in regard to the principle of the Uniformity of Nature—a principle which was widely circulated, a generation ago, as an experimental fact but which is now properly regarded as another formulation of the mechanical postulate. But, thanks to the metaphysician, this principle is now, so far as I know, regarded by us all, neither as an axiom nor as an emprical fact, but as a fundamental hypothesis which we may call the "uniformity postulate."

This assumption is practically equivalent to considering matter, energy and electrification to have no personal or individual traits which we need take into account. Without this postulate we should be unable to generalize our physical laws so as to include many new phenomena—phenomena unknown at the time of the formulation of the law. The tenacity with which the experimentalist holds to his assumption of a simple law is well illustrated by two papers read before the last meeting of this society: papers which illustrate how complex nature is becoming as research goes on. I refer to the work of Professor H. W. Morse and Professor E. B. Rosa on electrolysis. Each investigation dealt with slight deviations from one of Faraday's fundamental laws: and each investigation apparently assumed its truth: in any event assumed an equally simple law. Thousands of engineering results obtained each day in the week convince us that there are no accidents in history and allow us to believe that no postulate was ever better justified by its success.

The behavior of nature in this respect always reminds me of a remark, really a new formulation, once made by Professor Michelson in describing the labor of several years in locating and eliminating the errors in a certain steel rod upon which he was cutting an accurate screw. "I felt", he said, "as if matched in a game against an opponent: *but my antagonist always played fair.*"

II. ENERGY POSTULATE.

Passing now to the consideration of energy, it is not yet three score years and ten, since Poggendorff and Magnus refused space, in the *Ann. d. Physik.*, to Helmholtz's little tract, "Die Erhaltung der Kraft," on the ground that it was too metaphysical. But thanks partly to the clear vision of Helmholtz, partly to the clever analysis of H. Poincaré, and largely to the experimental success of the principle, the time has now come, I believe, when we can say that the conservation of energy is so useful, *as a postulate*, that present-day science can not successfully accomplish its work without it. Experiment

has been able to demonstrate it as a law only for particular cases and only approximately: but experiments have been so numerous and compelling, as to have created a new attitude of mind in the present generation, leading us to believe that everywhere in the physical universe there is *some* constant quantity, corresponding to a certain constant of integration, called "energy."

The most recent illustration of the manner in which the physicist assumes this constancy is, of course, the case of the steady heat production in radium. No sooner had Curie and Laborde made this remarkable discovery in 1903, than men began, *not* to doubt the validity of the law of the conservation of energy, but to look about for the energy which was thus being transformed into heat. Accordingly Rutherford and Barnes succeeded, in the following year, in showing that 23 per cent. of this intra-atomic energy was due to radium itself, 32 per cent. to radium C and 45 per cent. to the emanation and radium A together. In saying that the time has come when the Law of the Conservation of Energy may properly be regarded as one of the presuppositions of physics, it is to be carefully noticed that this statement does not include the Law of the Dissipation of Energy.

III. Causal Postulate.

The infinite regress involved in the search after causes and the vanity of attempting to follow a series of causes to its end are, at least, as old as the Greeks.

The postulate which the philosopher here shows us to be one of our presuppositions is as follows: events in physical science depend upon a few antecedents, knowing which we may successfully predict the immediate consequence, and may safely disregard all other circumstances. The brevity of the sequence which really determines phenomena in physics is a matter of continual surprise—while the length and complexity of the sequence in the case of ordinary human actions is a matter of equal astonishment.

But it is very easy to forget what a powerful influence this postulate has at times exerted in almost all departments of science. Few physicists, and still fewer engineers, of the present seem to realize that some of the most fundamental conceptions of our science have been introduced directly through the adoption of this postulate.

Take, for instance, what is perhaps the central idea of modern dynamics —the idea of force—an idea which is older than either that of mass or of energy. When viewed in the light of the causal postulate, *i. e.*, in the light of history, the definition of force becomes a matter of the utmost simplicity and perfect clarity. From many other points of view it is one of the most complex and puzzling of physical quantities. Sir Oliver Lodge says:

"We are chiefly familiar, from our youth up, with two apparently simple things, *motion* and *force*. We have a direct sense for both of these things. We do not understand them in any deep way, probably do not understand

them at all, but we are accustomed to them. Motion and force are our primary objects of experience and consciousness; and, in terms of them, all other less familiar occurrences may be stated and grasped."

To identify "force" in this manner with the "muscular sensation" of tension or pressure, which we feel when giving an accelerated motion to a body or when equilibrating by muscular effort the pull of the earth upon a body, seems to be dangerously near darkening counsel with words, and quite contrary to the spirit of the modern mathematician and physicist who are mending their fences at every possible point to keep out ideas which are not clear, sharp and definite.

The standard definition of the engineer, and, I fear, of not a few students of physics, is set forth by Professor William Kent in his article on the teaching of dynamics which appeared in *Science*[1] a few weeks ago, namely, "Force is defined as a pull or push, something that causes or tends to cause either a motion or a change in the velocity or direction of motion."

Now considering both of these points of view, which I believe are widespread, every one is willing to admit at once the existence of certain elastic and gravitational, and muscular, and electric, and cohesive, stresses which none of us understand: but the historical, or, if you please, the metaphysical, point of view would appear to be something like the following.

So far from our possessing any direct muscular sense of force, in the physical meaning of the word as distinguished from muscular tension, with which we are all familiar, the idea is one which was introduced by an Italian professor of mathematics, but a comparatively short time ago. How short may be illustrated by the following circumstances:

My grandmother, who lived in my own home for a number of years, was born on the banks of the Brandywine in 1789. She was therefore a contemporary as well as a neighbor of Benjamin Franklin. When Franklin was a printer's lad in London he had a promise from a friend that he should be taken to visit Sir Isaac Newton. Sir Isaac Newton was born within the same week in which Galileo died. Two human lives suffice therefore to bridge the gap between Galileo and our contemporaries. Back to Galileo is not therefore a far cry.

Recognizing the limitations of his science, and seeing that the search after causes was futile, Galileo adopted the causal postulate and prepared to confess his ignorance of gravitation, cohesion, muscular tension, and to say that, when we see a body changing its momentum, there is a "force" at work upon it. Following is the sentence, from his "Dialogues"[2] in which he introduces force as a synonym for any of these unknown influences which produce acceleration:

"It does not appear to me worth while to investigate the *causes* of natural

[1]Science, Vol. 30, p. 919, 1909.

[2]Ostwald's Klassiker der Exakten Wissenschaften, No. 24, p. 15.

motion concerning which there are as many different opinions as there are different philosophers. Some refer them to an attraction towards the center; others assign them to repulsion between the small particles of a body, while still others would introduce a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now all these fantasies, and others too, must be examined; but it is not really worth while. For all that is needful is to see just how one investigates the properties of accelerated motion and how these are defined, *without consideration of their cause*, in such a way that the momentum (of the body) increases uniformly from the initial condition of rest in simple proportionality to the time."

The paragraph which I have just quoted is, so far as I am able to learn, the earliest expression and definition of that central physical quantity which we now call "force." Observe first of all the modesty of the man; twice within this definition he inserts a distinct disavowal of any consideration of the cause of motion. So far is he in advance of our modern text-books, that he declines to define force as a "cause of motion" or a "tendency to produce motion," but says it is not even worth while to consider the question from that point of view.

How clear these same ideas were to Newton will be evident from the following two sentences from the first book of the "Principia." He says: "For I here design only to give a mathematical notion of those forces without considering their physical causes or seats." And again: "Wherefore the reader is not to imagine that by those words I anywhere take upon me to define the kind or the manner of any action, the cause or the physical reason thereof."

Having thus abandoned all consideration of cause and having assigned ourselves the simpler task of describing the motions of bodies, we come back to the definition of Galileo and Newton, namely, the rate of change of momentum—as the one perfectly *correct, competent* and *complete* description of force.

It remains only to show that Galileo had a clear and modern conception of momentum. This is sufficiently evident from the following paragraph in the "Dialogues."[1] He says: "It is clear than an impulse is not a *simple* matter, seeing that it depends upon *two* important factors, namely, the weights (il peso) of the colliding bodies and their velocities." And again on the same page he says: "It is costumary to say that the 'momentum' of a light body is equal to the 'momentum' of a heavy body when the velocity of the former bears to the velocity of the latter the inverse ratio of their weights."

If then I have correctly stated the facts of the case, force would appear to be a pure concept of the intellect: but a precious precept; one which is well

[1]Ostwald's Klassiker der Exakten Wissenschaften, No. 25, p. 44.

understood, clear, definite, quantitative, and one whose extraordinary usefulness has made it survive through the entire history of physics.

The paradoxy of this dominant idea of modern physics being a mere picture created by the human mind, disappears when we consider how the same method is employed in subjects other than physics.

In history, for example, we have important culminating events which we ascribe to "certain influences," while as a matter of fact the most that we actually know and observe in history is a series of *individual* acts, prompted, we suppose, by certain purposes.

The Franco-Prussian war came when the German Kaiser decided to send the telegram from Ems, when Prince Bismarck decided to publish certain parts of this telegram, when Von Moltke decided that the army was ready, when Napoleon III. decided to emulate the military career of his uncle, when the Congress of Vienna decided, in 1815, to give Prussia additional Rhenish territory, when in 868 the father of Lothar gave to his son the middle kingdom, the modern Lorraine, between France and Germany.

In practice we find it more convenient to say that "certain influences" had been at work for a full thousand years which culminated in the victory of Prussia over France. In physics, we give to the corresponding "influences" the name forces. That's the whole story! We *measure* these influences by the mass-acceleration of the body under consideration.

The extension which this idea of force has received in later times is shown to us all. Huygens was the first to show that Galileo's fundamental variable, linear momentum, might change in two ways, namely, in direction and amount; and he gives us for the first time a method of computing the force when the momentum varies in direction only—a force which we now call "centrifugal." Later, in the case of rigid bodies the conception of "angular momentum" was introduced; its time variation we now call either "torque" or "precessional couple" according as the angular momentum varies in amount only or direction only.

This definition is identical in form and meaning with that of Galileo.

The essential step made by Lagrange, in his treatment of the simplest possible case, namely, a single particle, is to derive both the time variation of momentum and the rate of directional change of momentum, each by differentiation of a single function.

Momentum for him is the velocity-variation of kinetic energy, a quantity whose time-variation is the tangential force; and centrifugal force is the space-variation of kinetic energy: but each of these is still a time-variation of momentum, agreeing perfectly with Galileo's original definition.

The space-variation of *potential* energy is the measure of stress—or more properly a stress integral—which we do not understand—but which nevertheless can be evaluated in terms of force.

I shall detain you for only one more illustration.

Faraday had discovered a quantity—the "electrotonic state," he called it—electrokinetic momentum, we call it—whose variations through any closed circuit, were always accompanied by an electric current in that circuit. Not knowing the cause of this current, physicists agreed to say that an "electromotive force" was at work whenever the electrokinetic momentum changed, and to define this electromotive force as the time rate of change of electrokinetic momentum (Neumann). Here again we have a generalized force introduced as a synonym for an unknown cause; exactly as was done by Galileo in the first instance.

Let us distinguish carefully between the observed facts of nature and those tempting pictures of the human mind which we only too easily create and are only too apt to worship.

Among the realities of mechanics are to be mentioned bodies in motion, liquids flowing, springs changing length; among the abstractions of the subject—helpful and needful abstractions—but abstractions nevertheless—are to be numbered the forces, velocities and accelerations of these bodies. Only by understanding these matters and by drawing a sharp line here shall we avoid Maxwell's "den of the metaphysician."

It is not infrequently that one finds a clever metaphysician in the orthodox man of empirical science; and I am free to confess myself unable to say whether the majority of the criticisms of the foundations of our science are due to the physicist or the philosopher; but in either case the critic speaks *as a metaphysician*. As an illustration consider the penetrating criticisms of the foundations of rational dynamics recently given by Mr. Norman Campbell,[1] who shows that the science of mechanics is so loaded with assumptions that the experiemental verification of its laws is utterly hopeless.

IV. Preliminary Discussions.

Fourthly, metaphysics, has I believe, rendered distinct service in giving us certain helpful preliminary discussions. Indeed, it is the history of many of the special sciences, such as psychology and sociology, that they were at one time departments of philosophy—but now, having shown themselves amenable to experiment or observation and subject to the "reign of law," are established as kingdoms of their own. The very notion of mechanical law is at least as old as Thales—600 B. C.—whose idea it was, in common with Anaximander, Anaximenes and Heraclitus, that the variety of things is due to "a single material cause, corporeal, endowed with qualities and capable of self-transformation."[2] Ridiculous and absurd as this sounds to us, it nevertheless contains the fundamental conception of mechanical law, and made it easier for later men to adopt more useful hypotheses.

[1]Phil. Mag., January, 1910.
[2]Ency. Brit., 23, 219.

The history of the atomic theory illustrates well the value of this contribution. The atom of Democritus—a purely metaphysical structure—differs in no essential respect from the modern atom up to the year 1738 when Daniel Bernoulli initiated the kinetic theory of gases.

The contention of Anaxagoras that all bodies are really continuous has also been of the utmost help: Poisson adopted it *in toto* in his mechanics; it was employed in electrical science up to the date of Helmholtz's Faraday lecture, 1881, and it is to-day practically adopted in all discussions of hydrodynamics. Maxwell[1] goes so far as to say: "In the earliest times the most ancient philosophers whose speculations are known to us seem to have discussed the ideas of number and of continuous magnitude, of space and time, of matter and motion with a native power of thought which has probably never been surpassed."

It was a really profound insight into the nature of pure mathematics that led certain participants in the relativity discussion, at the last meeting of this society, to place in the same class the metaphysician and the mathematician; the new grouping of studies at Harvard College does the same; each of these subjects is concerned neither with phenomena of any kind, nor with individual purposes, but with those over-individual purposes, with those universal agreements, with that world-wide consensus of opinion, in which all sane men unite; in brief, mathematics and metaphysics each belong in the group which Münsterberg calls the "normative sciences." There is therefore a certain sense, which in passing I merely mention, but do not urge, in which all consideration of number and quantity and limits which the mathematical philosophers have handed down, increases the debt of phy sicsto metaphysics.

Sound method in drawing inferences is a branch of science to which the physicist owns no copyright, but one in which he may claim to be fairly well versed. For this method he is indebted in no small degree to the development of logic in the hands of the metaphysician. In brief, modern physics, at its very inception in the seventeenth century, found that the schoolmen had already furnished it with a set of beautiful tools in the shape of fundamental logical ideas, including "precise definition," "classification," and "fallacies." Even Bacon, when "preaching the funeral sermon of scholasticism," used the accurate methods of the schoolmen.

Space and time, as continuous quantities and as limiting conditions for all phenomena, is another conception of no small value which we have inherited from the Greeks. The critical examination of our conception of time, which was given by Einstein[2] some five years ago, and perhaps even earlier by Lorentz, had, among other interesting and more valuable features, the following: He showed clearly—and, so far as I am aware, for the first time—just what

[1]Ency. Brit., art. Atom.

[2]Ann. der Physik (4), 17, 891–921, 1905.

kind of "time" we have been and are still using in ordinary Newtonian mechanics, namely, time such as would result from having all our clocks controlled by a single central time-keeper *which would transmit its controlling signals with absolute instantaneity.*

The clear definitions of synchronous clocks and simultaneity—in brief the idea of local time—may be considered as belonging either to physics or to mathematics—but surely the exposition in which Einstein has taught us just what kind of time we have been unconsciously using for more than two centuries is a metaphysical contribution of high order.

The dangers of mere nominalism, or, if you prefer, extrapolation, by which I mean the danger of ascribing to any physical system a set of properties which we have merely learned to associate with its name, has been clearly pointed out in the history of philosophy. Due regard for this warning would, I believe, have saved many pages that have been written concerning the ether —especially those devoted to a determination of its inertia, its weight, and its place in the periodic table of Mendelejeff.

V. Limitations of Science.

Fifthly and lastly, the metaphysician has rendered the inestimable service of pointing out to the experimental investigator the paradox that his greatest strength lies in his confessed limitations. Each of the particular sciences views phenomena from its own particular angle; but there is, I fear, sometimes—often, indeed—a tendency for the student of physics to think that in measuring, say, the inertia of a body, he is in some sense getting at the "quantity of matter" in it; or to put it in another way, there is often a tendency to think that in determining the mass, on a beam balance, he is perhaps doing something more fundamental than merely determining inferentially the ratio of the inertia of this body to the inertia of some body selected as a standard; for which purpose he has abstracted the inertia from all other properties of body and is really no nearer the nature of the ultimate "substance" of the body than if he had measured its temperature or its color.

A most important limitation which might have been entirely forgotten were it not for the metaphysician, is the fact that *phenomena* do not constitute the *entire* subject matter of science. Indeed it is only the mental and physical sciences which deal with phenomena. Human purposes and acts of the human will are quite as much subjects of scientific study, whether we consider the individual, the group or the entire race of sane men, as are any of the phenomena of physics. It includes such branches as history, politics, language and literature. Not only so, but if we define the real as that with which we must reckon in the accomplishment of our purposes, this second group of sciences deals with subject matter which is quite as real as anything we consider in physics.

It will perhaps not be out of place here to repeat the warning given by

President Maclaurin[1] to the American Chemical Society on the occasion of the recent Boston Meeting of the American Association for the Advancement of Science. He says: "We should pay more serious attention than we usually do to the logic of science and have as clear ideas as possible as to what we are really aiming at, as to what we can really expect to do and not to do. A little artificial stimulus toward philosophy might accelerate the process. It seems to me extremely unfortunate that men of science are still so much scared by the bogey of metaphysics. . . . We should realize, perhaps, that a science such as chemistry is above all else a work of art, and that concepts like atoms, energy and the like are not much more than pigments with which we paint our pictures."

Ether.—One other illustration must serve to complete this ungracious paragraph on limitations. I shall not weary you with citations from Lord Kelvin, telling us how much more we know about the ether than about ordinary matter, but I shall trouble you with a single sentence from that skilled expositor, Sir Oliver Lodge,[2] whose latest pronouncement upon this subject, omitting, however, the suppositions with which the entire argument is honeycombed, is as follows:

"The estimates of this book and of 'Modern Views of Electricity' are that the ether of space is a continuous, incompressible, stationary fundamental substance or perfect fluid, with what is equivalent to an inertia-coefficient of 10^{12} grams per c.c.: that *matter* is composed of modified and electrified specks or minute structures of ether which are amenable to mechanical as well as electrical force and add to the optical or electric density of the medium: and that elastic rigidity and all potential energy are due to an excessively fine-grained etherial circulation with an intrinsic kinetic energy of the order of 10^{33} ergs per cubic centimeter."

Suffice it to say that I am second to no man in this society in my admiration for that group of men whose names are associated with the following dates —1676, 1728, 1820, 1831, 1845, 1864, 1888, Römer, Bradley, Oersted, Faraday, Neumann, Maxwell, Hertz; names and dates which mark the discovery of the finite speed of light, the discovery of aberration, the discovery of the magnetic field produced by an electric current, the discovery of the electromotive force produced by magnetic displacement, the mathematical formulation of this result by Neumann, the combination of these two results by Maxwell and the prediction from them of electric waves, the experimental realization of these waves by Hertz. For brilliancy of achievement this series has certainly seldom, if ever, been surpassed in the history of physics.

But leaving matter aside, and considering the ether, what is the net result? Practically this, that *electromagnetic disturbances, including light waves, are propagated through space with a speed of 300 million meters per second.* This,

[1]Boston Herald, December 31, 1909.

[2]Ether of Space, p. 151, Harper, 1909.

I conceive to be the criticism which *every* sound metaphysician, but only *some* sound physicists, would pass upon our present knowledge of the ether. This is the one fact concerning the ether which we know in the same sense in which we are said to "know" the ordinary everyday facts of physics.

In conclusion, and still dealing with limitations, I beg to offer for your consideration a definition (*i. e.*, a delimitation) of physics recently given to me by an eminent metaphysician.

Last summer I had the pleasure of several times meeting Professor Münsterberg; and on one of these occasions I took the liberty of submitting to him, for criticism, a definition of physics, which I myself had formulated. Following is his definition of the physics of to-day which he, in return, submitted to me and which is, I am inclined to think, unsurpassed in point of accuracy, clearness and completeness: "Physics deals with changes in the world of over-individual objects, in so far as they are not changes of composition. It consists of those judgments which have proved themselves by trial to determine most accurately our justified expectations concerning these changes. In dealing with *objects* it separates itself from the knowledge of will-acts; in dealing with *over-individual* objects it separates itself from psychology; in abstracting from changes of composition, it separates itself from chemistry. The over-individual objects may be *matter* or *ether* or *electrons*."

The view of physics here presented is that of a half truth or partial truth. But this is very far from saying it is an untruth. The essential point—the only essential point—is for us to recognize the facts; to know ourselves; to admit our limitations. Then the more nearly we remain inside these limitations, and avoid "the den of the metaphysician," the better.

That flexibility of mind which it is desirable to secure *by not* translating every temporary opinion into a hard-and-fast fact of nature is well illustrated by a recent remark of Professor Schuster[1] who is himself one of the small group of men who have established the pulse theory of white light. "These two representations of white light (by homogeneous waves and by impulses) are," he says, "not mutually exclusive: They represent two points of view, and we may adopt either one or the other in different problems according to our convenience."

Less fixity and more flexibility in our views concerning the ether might, for instance, permit a more cordial consideration of Professor Osborne Reynolds's theory of gravitation which, so far as I understand it, has much to recommend it.

Conclusion.

Lest what I said at the outset concerning the experimental side of physics should be forgotten, let me, in justice to myself, remind you once more of my attitude toward the experimentalist, towards that group which in Italy includes Galileo, Volta, Melloni and Righi; the skillful group which includes

[1]Phil. Mag. (6), 18, 767, 1909.

Oersted, Kirchhoff, Hertz, Roentgen; the French group of laboratory workers, Mersenne, Fresnel, Regnault, the Curies; in England, Gilbert, Boyle, Joule, Rayleigh; and those dextrous men, our own countrymen, Franklin, Henry, Rowland, Michelson. Toward the experimentalist as compared with the friendly critic and reviewer, my feeling is precisely that of Lincoln toward the soldiers who fought at Gettysburg. You all remember his sentence—"The world will little note nor long remember what *we say* here; but it can never forget what *they did* here."

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THE ISOLATION OF AN ION AND A PRECISION MEASUREMENT OF ITS CHARGE.[1]

BY R. A. MILLIKAN.

THE method of measuring the elementary electrical charge which the author presented at the October (1909) meeting of the Physical Society[2] suffers from the limitation imposed by the gradual evaporation of the water drops. This makes it impossible to hold a given drop under observation for more than a minute, or to time the fall of a balanced drop through a period of more than 5 or 6 seconds.

This limitation has been entirely removed by the use of drops of oil, mercury and other non-volatile substances in place of those of water or alcohol, so that it has now been found possible to hold a single charged droplet under observation for 4 or 5 hours at a time, or even longer if desired.

The droplet is introduced into the space between the plates of a horizontal air condenser; it there catches one or more of the ions which normally exist in air or which have been produced in the space between the plates by any of the usual ionizing agents; its time of fall through a measured distance is observed when the field is off, then its time of rise through the same distance when the field is on. This operation is repeated and the speeds checked an indefinite number of times, or until the droplet catches a new ion when its speed under the influence of the field instantly changes, though its speed under gravity remains constant. From the sign and magnitude of the change in speed the sign and the exact value of the charge carried by the captured ion is determined. The error in a single observation need not exceed one third of one per cent.

In addition to furnishing the first exact determination of the value of the elementary electrical charge the use of the above method during the

[1] A paper presented at the Washington meeting of the Physical Society, April 22–23, 1910.

[2] Phil. Mag., February, 1910.

past three months has shown, (1) that Stokes' law in air breaks down for droplets having a radius less than .00013 cm., (2) that the majority of the ions of ordinary dust-free air both positive and negative carry the elementary electrical charge, (3) that, however, ordinary dust-free air contains some ions of both positive and negative sign which carry multiple charges.

The method is being applied to the study of ionization in gases other than air.

Some Investigations on Mica Condensers.[1]

By H. L. Curtis.

THE apparent capacity of a mica condenser depends on the method by which the capacity is determined. In general, the alternating current values are less than those determined by direct current measurements. By determining the apparent capacity at different periods of alternating current and plotting a curve, this curve may be extrapolated to zero frequency which will give the true or geometric capacity. Also, if the direct current capacity is determined with different times of discharge, the time of charge being the same, a curve may be obtained which will give the geometric capacity by extrapolation to zero time of discharge. The value of the geometric capacity by these two methods is the same.

An investigation was also carried on to determine the constancy of a mica condenser under different conditions. The results show that those condensers which are maintained at constant temperature and pressure may be depended upon to remain constant to one part in ten thousand, those which are kept at constant temperature vary by only two or three parts in ten thousand, while those which are exposed to the changes of room temperature may vary by several parts in ten thousand. Hence by keeping condensers under the most favorable conditions, the accuracy which can be obtained by their use is nearly ten times that which can be obtained when the condensers are left at room temperature and corrections applied to reduce to a standard temperature.

The Reliability of Magnetic Measurements on Rods.[1]

By Chas. W. Burrows.

THIS is a preliminary report on the results of an investigation on the nature and magnitude of the errors in magnetic measurements on comparatively short straight rods, as given by the methods and apparatus in common use in this country and abroad, including the Picou, Koepsel,

[1] Abstract of a paper presented at the Washington meeting of the Physical Society, April 22–3, 1910.

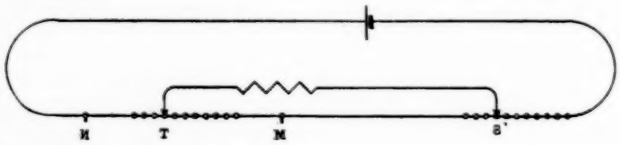
Esterline and Double-Yoke permeameters. These errors are due in part to the formation of local poles in the magnetic circuit which exert a magnetic field in the space occupied by the test specimen, and in part to the fact that the device for measuring the induction may indicate a considerable flux which does not pass through the specimen.

Curves and numerical data are given to show the corrections for the different styles of apparatus and also to show how these corrections depend on the nature of the test specimen. Nickel, soft iron, low carbon steel and high carbon steel of various cross-sections have been investigated and show characteristic shearing or correction curves.

NOTE ON POTENTIOMETER DESIGN.[1]

BY FRANK WENNER.

THIS communication relates to a device for the lower decades of a potentiometer to be used in the measurement of small electromotive forces. From the figure (which shows the arrangement of one decade) it will be seen that a part of the circuit including the potential point M is shunted by a comparatively high resistance. By means of a double dial switch both branch points between the shunt and the main circuit may



be shifted in steps of equal resistance so as to introduce a larger or smaller resistance between T and N while keeping the resistance shunted constant. The shifting of the switch then produces a change in the potential difference between M and N equal to the change in the resistance between T and N times the current in the shunt. The device works out well in the design of a low range two circuit potentiometer having a low resistance between potential terminals.

A METHOD FOR MEASURING OR ESTABLISHING THE TEN-TO-ONE RATIO OF RESISTANCE.[1]

BY FRANK WENNER.

THE resistance whose ratio is approximately ten-to-one and whose ratio is to be determined are made the adjacent arms (1) and (2) of a Wheatstone bridge. The other arms (3) and (4) are made up of resist-

[1] Abstract of a paper presented at the Washington meeting of the Physical Society, April 22–3, 1910.

ances so arranged as to form an approximate ten-to-one ratio. The bridge is then balanced by the variation of the resistance of one of the arms and a record kept of the condition producing the balance. The resistance in the arms (1) and (2) are then interchanged while the resistances in the arms (3) and (4) are put into a different combination (usually without a transfer of any resistance from one arm to the other) so as to make the ratio very exactly one hundredth of its former value. The bridge is balanced as before and from the two conditions of balance the ratio is easily calculated.

The ratio of the resistances in the arms (3) and (4) may be made approximately ten-to-one and then changed to very exactly one hundredth of the first value by the use of: (*a*) eleven nearly equal resistances, ten of which are used in the arm (3) first in series and then in parallel; (*b*) seven nearly equal resistances, five in the arm (3) and two in the arm (4) first with the five in series and the two in parallel and then with the five in parallel and the two in series; (*c*) six nearly equal resistances and one resistance of approximately one third the mean value of the others. In the latter case three of the nearly equal resistances are used in each arm first with those of the arm (3) in series and those of the arm four in parallel and the smaller resistance in with the series combination, and then with the conditions just interchanged. Here, since the smaller resistance is transferred from one arm to the other, a correction must be applied unless it is very exactly one third the mean of the other six.

A Condition Limiting the Precision Attainable by Increasing the "Order" in the "Method of Diameters" [1] for Wave-Lengths.[2]

By Irwin G. Priest.

THE author emphasized a fact which he said seemed obvious, but had not been emphasized by the investigators who had dealt with matters in which it is involved. Some authors have indeed tacitly assumed a condition out of accord with this fact.

If, in a determination of wave-length, p is the order of interference and δp is the error in p resultant from the errors of observation and the method of computing, then the relative error in the wave-length is directly proportional to $\delta p/p$. In the "Method of Diameters" p is evaluated from the relation

$$p = \frac{N}{\cos x/2},$$

[1] Fabry & Buisson, Jour. de Phys. (4), 7, 169, 1908. Eversheim, Ann. der Phys., 30, 815, 1909. Pfund, Astrophy. Jour., 28, 197, 1908.

[2] Abstract of a paper presented at the Washington meeting of the Physical Society, April 22–3, 1910.

N being known and the angle x or some function of it being the experimentally measured quantity. It can be shown that

$$\frac{\delta p}{p} = \frac{\sqrt{p - N}}{\sqrt{2}\sqrt{p}} \cdot \delta x.$$

Hence, if δx, the error in x, is constant and $(p - N)$ is constant, $\delta p/p$ varies inversely as $\sqrt{p}$.

It has been proposed to improve the present relative accuracy in the measured wave-length by using larger values of p. In view of the relation just pointed out, this procedure does not promise much, for to produce the desired reduction of $\delta p/p$ such large values of p would need be taken as to seriously increase the experimental error δx, which becomes large for very large values of p.

WASHINGTON,
April, 1910.